THE STORY OF THE SUN

SIR ROBERT BALL

PLATE I.

THE SUN, SHOWING SPOTS, FACULÆ, CHROMOSPHERE, AND PROMINENCES *(Trouvelot)*.

THE
STORY OF THE SUN

BY

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Author of "The Story of the Heavens" and "Star Land"

WITH ELEVEN FULL-PAGE PLATES
AND NUMEROUS ILLUSTRATIONS

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LA BELLE
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PREFACE.

I HAVE to acknowledge, with many thanks, the aid which has been rendered to me in the preparation of this book:

By Professor Piazzi Smyth, in permitting me to have a plate copied from his original drawings of the Total Eclipse at Bue Island. By Mr. Higgs, who has given facilities for the reproduction of his beautiful photographs of the Solar Spectrum. By Professor Pickering, in enabling me to avail myself of the publications of the Harvard College Observatory. By Professor Holden, in according a like permission with respect to the Lick Observatory. By Professor Janssen, in lending me beautiful photographs of Sun-spots. By Rev. F. Howlett, in supplying original drawings of solar phenomena. By Mr. Mollison, in giving me a copy of Galileo's drawings. By M. Flammarion, in allowing me to use illustrations in *L'Astronomie.* By Professor Hale, in facilitating my account of his remarkable work. By Mr. Ranyard, in placing at my disposal various illustrations from *Knowledge;* and by Miss A. M. Clerke, Dr. G. Johnstone Stoney, Rev. M. H. Close, and Dr. Rambant, in reading the proofs.

ROBERT S. BALL.

CAMBRIDGE, *November*, 1893.

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The Story of the Sun.

CHAPTER I.

THE SOLAR SYSTEM.

IT will be necessary for us at the outset of this work to explain with a certain amount of detail the principal features of that remarkable system of bodies of which the Sun is the centre. The great luminary is, indeed, the centre, in many other senses besides that in which we attribute to it a certain geometrical position amid the orbits of the several bodies which revolve around it. It is to the control of the Sun that the several planets are indebted for the regulation of their movements, and their retention at distances from him which are nearly constant. It is, of course, the same great body which dispenses so liberally to his attendant orbs the light and the heat, without which it would be impossible for any of them to form inhabited worlds—at least, in any sense in which our experience would interpret such a phrase. It will appear that it is impossible for us to solve the very first problem suggested in the consideration of the Sun without calling to our aid information only to be obtained from some of the other orbs by which he is attended. The primary question naturally proposed in any discussion of the physical characteristics of our great luminary is what his dimensions may be. We desire, in fact, to learn the number of miles in his circumference or in his diameter; and it is also needful for us to ascertain with all practicable precision the actual mass of the Sun as compared, let us say, with a globe like our Earth. For the determination of these fundamental elements we are indebted to certain measurements which can be made on the positions of the planets; indeed, we may assert that if our system was of that extremely simple type represented by a solitary body revolving around a central Sun—that single planet being, of course, the Earth—it would hardly have been possible for us to have ascertained the distance of the Sun, or to have measured his mass with anything like the precision that has been found to be attainable when other planets render their assistance. This is the chief reason why some description of the system which surrounds the Sun cannot be omitted from a book which treats of the great luminary itself. In this preliminary chapter we set forth such information as may be necessary.

From the remotest antiquity it has been recognised that there were five planets belonging to the Solar System—namely, Mercury, Venus, Mars, Jupiter, and Saturn. It is true that the physical relation of these globes to our system was not correctly understood until comparatively modern times. The movements of the objects which bore these names seem, however, to have indicated, even to the simplest type of observation, that these bodies are to be placed in a wholly different category from the fixed stars, properly so called. It may be interesting to note that even Mercury, which is by far the most difficult of detection of all these orbs, did not escape the penetrating glance of some early discoverer, whose name, and even that of the nation to which he belonged, are utterly lost in pre-historic ages. It may also be remarked that so far as we are at present aware, the knowledge of these five planets was confined to the various civilised nations of the Old World. It would seem that the Aztecs of ancient Mexico, notwithstanding the advances that they had made in the study of certain astronomical phenomena, had not noticed any of the planets except Venus. The movements of Mercury, Mars, Jupiter, and Saturn had not, so far as our information goes, attracted their attention. These great globes were doubtless thought by such observers to differ in no marked degree from the bright stars which they superficially resemble. No words, however, could be too strong to emphasise how vast is the intrinsic difference between what we now understand by the word planet and what we understand by the word star. Of the bodies which belong to the Solar System, the nearest to the Sun is the planet Mercury, which performs its circuit around the great luminary in 87·969 days. The shape of the path which Mercury pursues may be taken to illustrate the shape of a planetary orbit in general. The average distance of this globe from the Sun is 35·9 millions of miles, but the orbit is not a circle, and consequently the distance is sometimes greater than this, and sometimes less. When at their least distance apart the interval between the two bodies is 28·6 millions of miles, while at their greatest separation it is no less than 43·3 millions of miles. Venus comes next in point of distance, revolving in a nearly circular orbit at an average distance of 67 millions of miles—then follows the Earth, its mean distance being 92·7 millions of miles. Outside the Earth revolves Mars, averaging an interval of 141 millions of miles from the Sun. In mentioning this planet, we may here note that its orbit, like that of Mercury, departs somewhat considerably from the circular outline, the least distance being 128 millions of miles, and the greatest distance 155 millions. Next to the orbit of Mars lies that of Jupiter, at an average distance of 482 millions of miles from the Sun; and then follows Saturn, the last of the planets known to the ancients, circling in a path that was formerly regarded as the frontier of the Solar System at a mean distance of 885 millions of miles from the Sun.

Each mighty journey of Saturn around its path is accomplished in a period of 10,759 days, or nearly thirty years. The figures which we have here given, with a few other important numerical quantities, will be found set forth in the appendix to this volume.

In the adjoining figure (Fig. 1) we have laid down the orbits of the four inner planets to scale. It would not be possible within the compass of the same map to represent, in their just proportions, the paths pursued by all the different planets; accordingly, in the next figure (Fig. 2) we have delineated the orbits of the outer planets, the interval between Mars and Jupiter separating what we know as the inner group from the outer. In these figures the departure of the forms of some of the orbits from the exactly circular type is sufficiently apparent. It has been already mentioned that in the cases of Mars and Mercury the ellipticity of their paths is especially pronounced; while, on the other hand, in the case of Venus the departure from a true circular shape could hardly be perceived without very exact measurement.

In ancient days it was maintained that circular revolution, being—as was regarded—the only type of perfect movement, must necessarily be that in which celestial bodies made their journeys. When, however, exact observations had been sufficiently accumulated, it became apparent that the revolutions of the planets around the Sun were not performed in exact circles. One of the great epochs of astronomical history is marked by Kepler's discovery of the true shape of the track in which a planet moves.

Among curves which attract us by their geometrical beauty, there is none so interesting as the figure which we call the ellipse. It can be drawn in many different ways, perhaps the simplest being that so often employed for many practical purposes. Two pins are stuck through a sheet of paper on a drawing-board, a loop of string is passed over them, and then the point of a pencil, which keeps the loop stretched, traces out the ellipse. The different varieties of this curve may be drawn by altering the distance between the two pins while preserving the length of the loop unaltered. If, for instance, the two pins be brought close together, then the form of the curve departs but little from the circular outline; if, on the other hand, the pins be separated so widely as to leave but little slack in the loop, then the shape of the curve is a very elongated oval, or, as a geometer would express it, an ellipse of high eccentricity. Mathematicians have always found the properties of the ellipse to offer most interesting matter for their investigations. Many beautiful discoveries about the geometrical characteristics of this class of curves had been made long before Kepler's time. In fact, when he essayed to explain the movements of the planets, he was enabled to use the ellipse which he found, so to speak, ready prepared to his hand by many centuries of geometrical research. Having ascertained that the circular path could not in any way be made to represent planetary movements, it was natural to

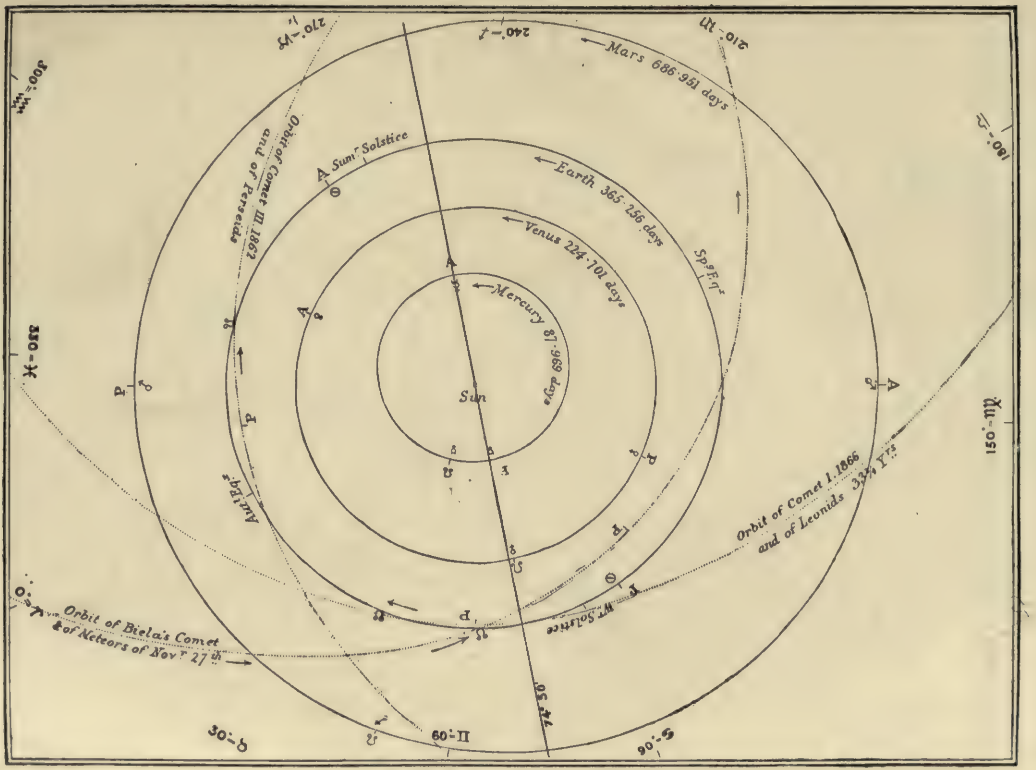


FIG. 1.—ORBITS OF THE INNER PLANETS.

try that curve which, next to the circle, had chiefly engaged the attention of geometricians; and after much labour, and after many failures, he at last succeeded in establishing that every planet revolves around the Sun in an elliptic path. It may be that the ellipse is nearly a circle, as in the case of Venus, or it may be that the elliptical orbit of the planet departs as widely from a circle as do those followed by Mars or by Mercury, or even by certain other planets which move in orbits of still greater eccentricity; but the law which bears Kepler's name affirms that under all circumstances the path must necessarily be an ellipse of one form or another. This, surely, is a most striking result; it at once attributes to these beautiful curves, so interesting to the geometer on other grounds, a unique function in the interpretation of Nature.

But we have as yet stated only one of the discoveries which rewarded Kepler's endeavours to explain the movements of the planets. At first it might have been thought that the Sun would most naturally find its place at the centre of the elliptic highway which the planet traversed; but Kepler speedily found that such a position could not be attributed to the Sun, if the observations of Tycho Brahe, on which he depended, were worthy of confidence. Here was, indeed, a certain want of symmetry, for the centre of the ellipse is clearly the only point quite symmetrically disposed with regard to the contour of the curve. It was obvious that the position which the Sun actually occupied must be one of much physical importance, and it was natural to expect that when the point was found it would be one that possessed a geometrical significance also. Next to the centre, the most remarkable points in their relation to the figure are clearly those which the mathematician calls the two foci. They are, indeed, the situations occupied by the pins in the process of drawing the ellipse which I have described. The geometrical significance of these points is obvious; you cannot find more than one pair of foci for one ellipse, for if you try any two different points within the circuit of the curve, except those originally employed, you will find that, no matter how you adjust the loop of the string, it will be impossible to reproduce the same ellipse. A happy inspiration then

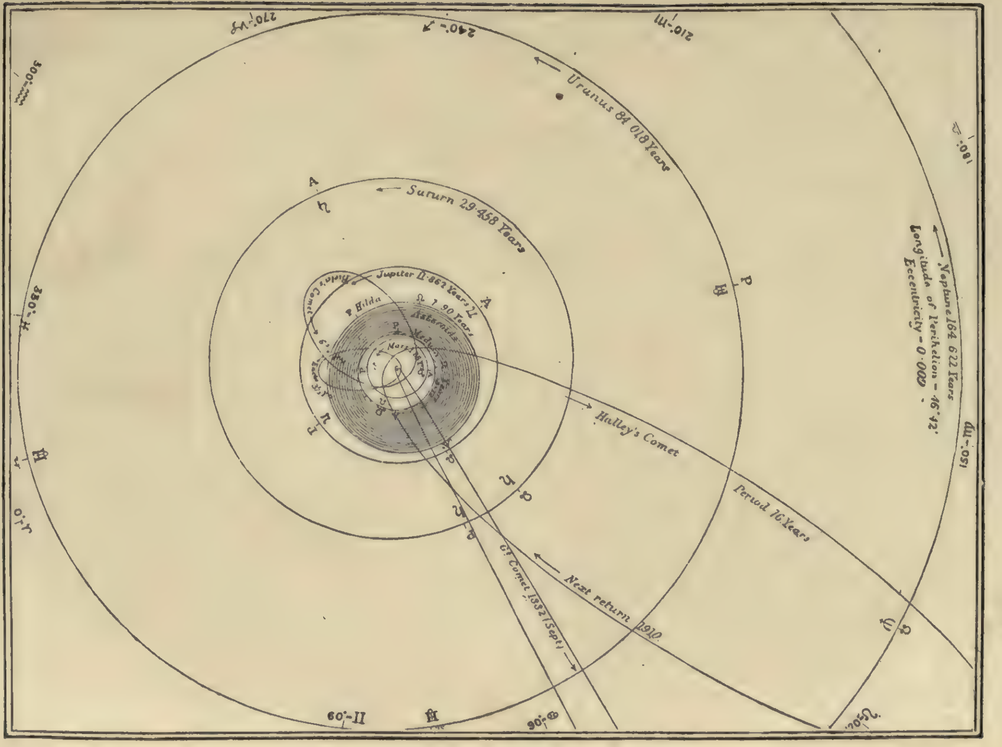


FIG. 2.—ORBITS OF OUTER PLANETS.

suggested to Kepler that as the Sun certainly occupied some position in the interior of the curve, and as that position was not the centre, it might possibly be one of the foci. Great, indeed, must have been the delight of the astronomer when he found that this anticipation was verified. No sooner had he regarded the Sun as situated at one of the foci of the ellipse, and the planets as revolving in the circumference, than all became clear, and the first great problem of the movements of the Solar System had been solved. But a further point remained. If the planet revolved in a circle, the principles of symmetry would seem to declare that the velocity with which that movement was performed must remain constant. When, however, it was found that the orbit was not a circle, but an ellipse, it did not then seem to be required, from considerations of symmetry, that the pace at which the planet moved should be invariable all round the track. As a matter of fact, Kepler speedily discovered that the actual pace of the planet varied in a very interesting manner. I have represented an elliptic orbit in Fig. 3, and I have marked at seven places the corresponding velocities which would be appropriate. The illustration is, however, rather intended to exhibit planetary motions in general than the particular circumstances of any one planet. It will be seen that at the remotest part of the track from the Sun the speed marked is five miles a second. As the planet moves on towards that end of the orbit which lies nearest the Sun its pace gradually accelerates to seven, ten, and sixteen miles a second, until, when sweeping round that part of its course in the immediate vicinity of the attracting body, the velocity attains its highest value—twenty miles a second. From that point onwards the speed begins to abate; it declines to sixteen, and to ten, and to seven miles, as the planet gets further and further away, until at last, when the body has regained its original position at its greatest distance from the Sun, the speed has returned to five miles a second, with which we supposed it to commence.

It will be noticed from this illustration that the nearer the planet is to the Sun the quicker is the pace at which it moves. Such is, however, not the accurate form in which we express what is called Kepler's Second Law of Motion. The correct way of enunciating this important doctrine with regard to the movement of the planets, is to say that *the line joining each planet with the Sun describes equal areas in equal times.* Thus, for instance, in the picture already given, the shaded portions represent the areas described in equal periods of time. In its journey from A to B the planet sweeps over the area A S B, and it describes the area thus indicated in a certain number of days. In this case, the planet is at a long distance from the Sun, and consequently the arc A B has to be a short one, so that, due regard being paid to the lengths of S A and S B, the area performed should be the correct one for that period. On the other hand, when the planet is moving from P to Q, in its description of the arc P S Q, the body is then so near the Sun that, to make the area P S Q as large as the area A S B, the arc P Q has to be greatly longer than A B, which is traversed in the same time. Therefore, while moving from P to Q the pace has to be much higher than when the body is moving from A to B. Suffice it now to say that the application of this law decides for each point of the orbit the actual speed with which the planet must be then animated.

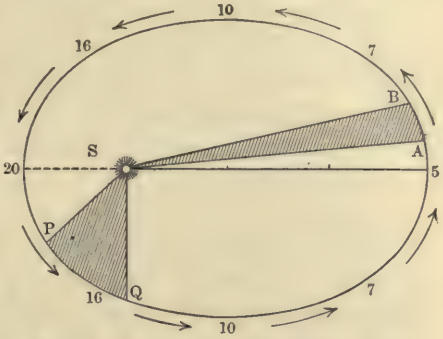


FIG. 3.—TO ILLUSTRATE KEPLER'S LAW.

We have yet to mention Kepler's Third Law, in which he demonstrated the relation which exists between the mean distance of a planet from the Sun and the time necessary to complete an entire revolution. This introduces the comparison between the movements of one planet and those of another. This Third Law is expressed as follows. *The squares of the periodic times of the planets are severally proportioned to the cubes of their mean distances.* From this it follows that if we know the periodic time and the mean distance of any one planet, then we are able to learn by simple calculation the mean distance of any other planet of which the periodic time has been previously ascertained. The three Laws of Kepler exhibited in the completest manner the principal characteristics of planetary movement round the Sun. They might be regarded as constituting a concise way of expressing certain remarkable facts established by the observations of the astronomers who preceded Kepler. But a further great step was necessary before these laws should attain the real natural importance for which they were destined; as they left Kepler's hands, even these great truths could only have been described as empirical; they were the result of assiduous observations, and they were demonstrated to be correct by the fact that they were invariably confirmed by further experience. Kepler's Laws had, however, then no higher claim on acceptance. They offered no reason why, in the nature of things, a planet should actually move in an ellipse rather than in any one of the many hundreds of other curves which geometers either knew already or could easily conceive.

Kepler's discoveries, interesting and instructive as they are, would never have attained their present importance had it not been for the far greater discoveries to which they conducted Newton. To his immortal glory, he demonstrated that Kepler's Laws were themselves a consequence of the Law of Universal Gravitation. Starting with the fundamental supposition that *every body must attract every other body with a force which varies directly as the product of their masses, and inversely as the square of the distance by which they are separated*, Newton proved that a planet in revolving around the Sun must necessarily describe one of that class of curves to which Kepler had shown its orbit to belong. Here a new significance was given to Kepler's Laws. It was made clear that if the planet moved in an ellipse, with the Sun at its focus, then there could be no other explanation of this movement, save that there must reside in the Sun an attracting force, varying according to the inverse square of the distance. Moreover, the further details of the movements of the planets, as indicated by the two remaining Laws of Kepler, were also seen to be necessary consequences of the principle of Universal Gravitation.

Kepler's Laws have an immediate bearing on our present subject, inasmuch as they are intimately connected with the determination of the distance of the Sun from the Earth. Let us first note that it is comparatively an easy problem to ascertain the periodic times of the several planets. Consider, for instance, the planet Venus, and see how, even without any refined measurements, its periodic time can be found. We note the time when it is brightest as the evening star, and we can recall the last occasion on which the same phenomenon occurred; the interval between these, though not indeed the actual time of Venus's revolution around the Sun, will enable that period to be learned by a simple calculation. It would not afford any very accurate evaluation if we merely determined this result by observing a single interval between two successive brightnesses of Venus. But suppose we look back for a century, or for many centuries, and count, as it is quite easy to do, the number of such returns of the planet and the interval of time that they have required, then from the average of all, the slight variation incident to each particular return disappears, and we are enabled to determine the periodic time of the planet with all desirable precision.

In like manner, the periodic times of Mars or Mercury, Jupiter or Saturn, can be learned with the necessary exactness by comparison of modern observations with those of antiquity. The importance for our present purpose of such determinations will now be obvious. Have we not already seen by Kepler's Third Law that the periodic times and the corresponding mean distances of the planets are definitely connected? When, therefore, we have determined the periodic times from such calculations as I have just mentioned, we are enabled to discover the relative values of the several mean distances. Thus we know at once the shape, so to speak, of the Solar System. We can draw a map, in which the several orbs are depicted at their true relative distances, and all that is wanted in order to learn the absolute value of the several dimensions in our system, will be to find out any one magnitude by which the required scale will be given. For instance, what we specially want to learn is the distance of the Earth from the Sun. If we knew the distance of Venus from the Sun, then by the known proportions of the map we should be able at once to find the distance of the Earth from the Sun.

In fact, the case is like one which often arises in the use of an ordinary atlas. Suppose you have a map of Europe which happens not to be provided with a scale showing how many miles go to the inch. You can, nevertheless, speedily ascertain the distance between any two places, provided you are acquainted with the distance between any other two places on the same map. For instance: suppose we want to find the distance from London to St. Petersburg. We may first of all measure the distance from London to St. Petersburg in inches on the map; and then to find the value of one inch, we take some known interval, say from London to Edinburgh, the distance of which is four hundred miles. If these two cities should be one inch apart on the map, then we may know that one inch on this scale corresponds to four hundred miles, and that therefore the distance from London to St. Petersburg being $3\frac{2}{3}$ inches must be interpreted to mean one thousand four hundred and seventy miles.

I have hitherto only spoken of those planets which, being conspicuously visible to the unaided eye, have been known from all antiquity. It will now be necessary to mention that the Solar System, as we understand it in these modern days of large telescopes, is a vastly more complex system of organised movements than was presented to Kepler when his great labours were undertaken. In the first place, Saturn has been now, for more than one hundred years, deposed from the position it once occupied as the outermost of the known planets. Outside that globe revolves the great planet Uranus, which, at a mean distance of 1,777 millions of miles, nearly twenty times as great as that of the Earth, performs its mighty revolution in a period of eighty-four years. Though Uranus is so vast that its diameter is more than four times as great as that of the Earth, yet it is an object only to be discerned by the unaided eye under very favourable circumstances on an exceptionally clear and dark night. In fact, this planet so closely resembles a star that it was never distinguished by the acuteness of ancient astronomers from the tens of thousands of similar stellar points. It was reserved for William Herschel, with his home-made reflecting telescope, to bring the planet Uranus to light by his superb discovery of this great globe in 1782. For the lapse of another half-century it seemed as if Uranus was to be entitled to the position that had been so long accorded to Saturn of circumscribing the limits of our system. But in 1846 Uranus was itself superseded. The discovery of Neptune is a well-known chapter in astronomy; I have so fully described it elsewhere that I do not propose to enter into it here. Suffice it now to say that the planet Uranus indicated by its movements the existence of some great orb still more remote. The annals of astronomy present no more brilliant achievement than that by which Le Verrier and Adams, guided solely by such indications as the movements of Uranus afforded, were conducted to a knowledge of the precise spot in the heavens where a planet lay on which no human eye had ever gazed. The telescopic discovery of this planet, made solely in consequence of these theoretical indications, was the crowning point of the Newtonian doctrine of Universal Gravitation. Neptune is the name of the vast orb so revealed; it is entirely beyond the scope of the unaided eye. A telescope is required to show it, and then the planet appears like a star of about the eighth magnitude.

Before going further, it will be necessary to mention the principles by which a planet in actual observation is to be discriminated from a star. To the unaided eye there is no test that can be applied of a thoroughly reliable character, except that of the presence or absence of movement. Jupiter and Venus, no doubt, under certain circumstances shine more brightly than any ordinary star; but Saturn and Mars might easily be mistaken for stars—as indeed they often are. To some eyes, it is true, the difference in character between the light from a planet and the light from a star is more or less recognisable. No sound means of discrimination between the two, however, can be procured from this consideration. The only reliable criterion available to the unaided eye is provided by the fact of the planet's movement. If the body believed to be a planet be aligned with the fixed stars in its vicinity, it becomes possible, in months or weeks, or sometimes even in days, to detect the movement if it exists, and thus to disclose the real character of the object. But to those who are observing with a telescope, the contrast between a star and one of the more important planets is immediately recognised. Whatever may be the power of the instrument, the star appears only like a point of light—a point of sun-like brilliance, it may be, but still devoid of appreciable form. Quite otherwise, however, is the telescopic appearance of one of the chief planets. The instrument displays at once to the observer a more or less globular body, whereof he is not only able to see the outline, but even, in many cases, to discriminate certain features or markings on the disc. In examining Venus, for instance, it is the beautiful crescent form of that body when seen as an evening star or a morning star which first rivets attention. The moon-like appearance of this globe is indeed one of the most striking features disclosed by the telescope in the Solar System. In a less conspicuous manner the planet Mercury also exhibits phases of a moon-like character. Mars presents to the observer a globe marked over with features

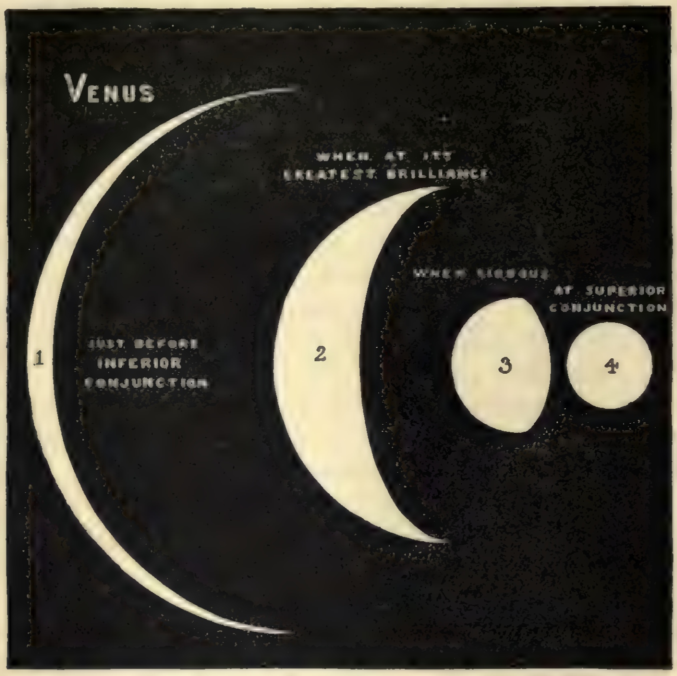


FIG. 4.—DIFFERENT PHASES OF VENUS.

so far capable of being distinguished that maps of his surface, and even globes representing it, have been constructed. Jupiter, the largest planet of all, shows belts and other markings on his surface indicative of the mighty system of clouds by which his great bulk is encompassed. Of all the planets, however, Saturn competes most successfully for the attention of the possessor of a good telescope. This famous planet exhibits a globe of such stately proportions that it is more than seven hundred times as big as our Earth. No doubt certain features are to be remarked even on the ball of Saturn, but anything that can be thereon discerned is greatly inferior in interest to the phenomena which can be seen on the other great globe, Jupiter, which is at once larger than Saturn and much nearer to the terrestrial observer. But the feature which gives to Saturn a unique interest is the marvellous ring, or rather system of rings, by which its globe is surrounded. Next outside Saturn we come to Uranus, but the interest of this object as a telescopic spectacle is very much less than are those presented by the other

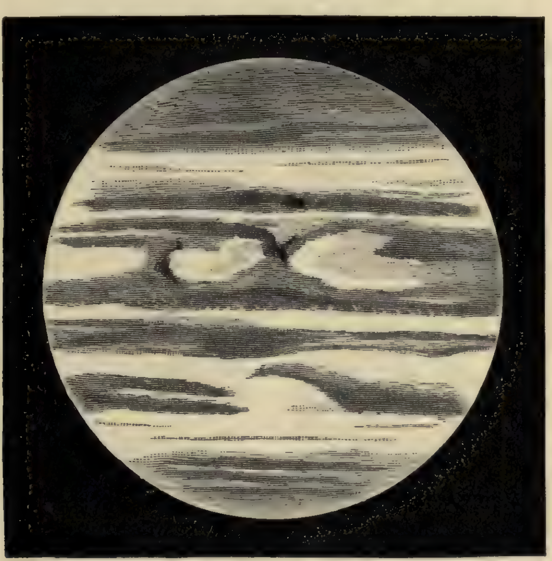


Fig. 5.—Jupiter.

great planets we have mentioned. Notwithstanding the mighty globe which Uranus no doubt possesses, it presents but little that is noteworthy so far as its features are concerned. There is, indeed, a double reason for the apparent insignificance of Uranus. In the first place, the remoteness of this planet is such that from our point of view its mighty bulk seems reduced to very minute dimensions, though it is still sufficiently large to be discriminated from a star by the telescopic visibility of a disc. But that this disc is by no means obvious without a fairly good telescope will be evident from the circumstances which were brought to light after its discovery had been announced by Herschel. It then appeared that the planet had been looked at no fewer than seventeen times by even experienced astronomers before it came under Herschel's notice. No expectation of a visible disc on such a body had been present to their minds, and the actual disc which the body possessed was not broad enough to strike unexpectant observers. They merely regarded the planet as one of the countless host of stars, and recorded its position in the star-catalogues which it was the object of

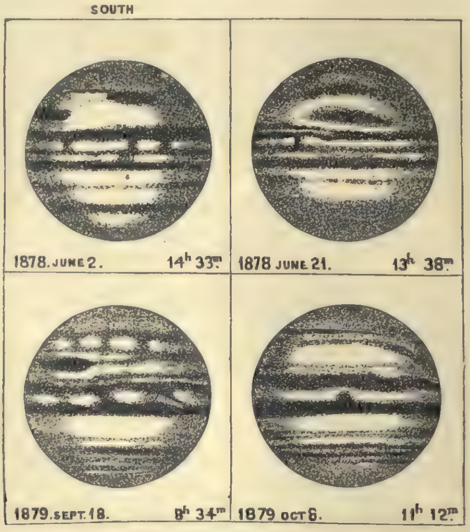


FIG. 6.—VIEWS OF JUPITER.

their researches to produce. How little were these early astronomers conscious of the wonderful discovery which lay so nearly to their hand! It was in consequence of the exceptional acuteness of Herschel's powers of observation, that when this planet entered into the field of his telescope his scrutiny showed that it had a different aspect from that of a fixed star. The attention of the great astronomer was accordingly quickened, and he watched the mysterious body narrowly. Further observation disclosed that this object was in actual movement, this circumstance alone clearly discriminating it from an ordinary star. Ere long Herschel demonstrated that what he had found was indeed a majestic planet: the first of all the bodies so called which have been discovered within the memory of man.

But there is another reason why a planet so remote from the centre of the system as Uranus should appear faint to the terrestrial observer. It must be remembered that all planets resemble our Earth in the circumstance that they are indebted to the radiation from the Sun for the light which they receive. The intensity of the Solar beams on a planet are, of course, inversely proportional to the square of its distance from the Sun. Consequently, we find that the actual brightness of sunlight on the surface of Uranus would only be the 369th part of the brightness we enjoy on the Earth. Daylight on Uranus, could we be transferred thither, would therefore make a very poor contrast to the effulgence to which we are accustomed. Thus it is that the brightness of Uranus, illuminated as that planet is by only a minute fraction of the lustre shed upon the Earth or Moon, must necessarily appear very small, especially after the further reduction incident to the fact that we are looking at the planet across such a vast interval.

Much of what we have said with regard to Uranus may be applied with slight modification to Neptune. Though that planet is so large that ninety-nine bodies equal to the Earth incorporated into one globe would hardly equal it in size, yet it is so much farther than even Uranus that the difficulty of satisfactorily observing it is correspondingly enhanced. Its disc is extremely minute, owing to its remoteness; while the sunlight by which

Neptune is illuminated is, of course, fainter still than that which falls on Uranus. For these reasons Neptune is invisible to the unaided eye. When viewed, however, through a powerful telescope, the characteristic planetary disc is quite recognisable, though much more favourable conditions of seeing are required than those which would suffice to make the disc of Uranus visible.

To complete this very brief account of the Solar System; we have now to add that there are within its limits a vast number of planets of much smaller dimensions than those already mentioned. The majority of these little bodies revolve in that wide space bounded on the inner side by the orbit of Mars, and on the outer side by the orbit of Jupiter. These asteroids, as they are generally called, are, with the exception of Vesta, entirely telescopic. The larger members of the group may no doubt be several hundred miles in diameter; but the majority are composed of little objects, many of which are apparently not much more than twenty miles in diameter, or perhaps even less. Objects so minute, situated at distances which, even at the lowest, are not less than thirty or forty millions of miles away, must necessarily require telescopes of exceptional power to render them visible. In the case of such tiny points of light, it is never possible to apply the criterion as to the presence or the absence of a disc for the purpose of discriminating between these little planets and the stars which they so closely resemble. The only method by which their planetary character can be certainly revealed is by following the movements by which they change their places on the heavens. Nor is the application of this criterion by any means a simple matter. The small stars are strewn in hundreds of thousands over the heavens; how, then, are we to discriminate among all these legions the small planet here, or the small planet there, which may be slowly advancing or retreating. Until recent years, the only method available involved the preliminary preparation of charts of the heavens, on which the myriad stellar points were laid down with the utmost attainable accuracy. The discovery of a minor planet was then effected by a careful comparison between the star-like points on the sky, as disclosed through a good telescope, and the corresponding points on a chart, constructed, perhaps, some years before. Occasionally it would be noticed that an apparently stellar point in the heavens would not be indicated by any corresponding point on the map, and then the attention of the diligent astronomer would be aroused. He would watch this object, which even in an hour or two would often sufficiently betray its character by its movement, and thus a new planet would be added to the list. In this way the number of these bodies has been gradually augmented. Within the last few years a somewhat less tedious process has been applied with great success. It is now possible to dispense altogether with the laborious preparation of a map by the old process of charting, and the almost equally laborious comparison of the chart point by point with the sky. Photography has rendered a much simpler method available. A sensitive plate is placed at the focus of the telescope, and the instrument is moved synchronously with the revolution of the heavens, so that the rays from each star shall be kept constantly concentrated at the same point of the plate. After an hour's exposure the operation is over, and the picture is developed. If the process has been successfully carried out, each of the stars, to the number, it may be, of hundreds or of thousands, will show on the photograph as a definite round image. Should it have happened that an asteroid was at the time passing through that part of the heavens which had been photographed, the character of the object would be disclosed by its movement. During an hour's exposure the asteroid would have changed its place on the heavens to an extent quite appreciable. The consequence is that while each of the stars, properly so called, manifests itself as a disc, the planet is represented by a little streak on the plate. In this way the character of the object is discriminated from the adjacent stars, and a new asteroid may be brought to light. It has, indeed, happened that two, or even three, new planets have been detected by this method on a single plate, on which were also imprinted several other planets, whose existence had been previously made known by the earlier methods of investigation. To illustrate the fertility of the photographic process for the discovery of asteroids, it may be mentioned that in the course of the year 1892 Herr Wolf discovered a planet by this method on the 22nd of August, another on the 1st of September, three on the 25th of the same month, and one on October the 20th. Other observers were also rewarded during the time specified by discoveries of a similar kind.

M. Charlois, too, has discovered many new planets on negatives taken since September 12th, 1892, and it is especially interesting to note that the apparatus he employed was of a comparatively simple character. It was, indeed, no more than an ordinary portrait-lens of large aperture, provisionally fixed to the equatorial stand of a great telescope, so as to enable the requisite movements to be imparted with facility. So assiduously is the search now maintained that there is good reason to believe that not many asteroids above the fourteenth magnitude can remain much longer undetected. Doubtless, with longer exposures of the plates it might be possible to obtain pictures which would betray the presence of bodies of a planetary character much more minute than any of those hitherto recognised. In fact, just as photography has revealed the existence of many stars too minute to be visible to the clearest eye even with the most powerful telescope, so it seems not unlikely that the same method may avail to disclose planets which are small enough to elude vision with any telescope whatever.

Astronomers are just commencing to reap the advantages of photographic methods in the study of the smaller bodies in the Solar System, and for our present purpose the cultivation of this branch of research is eminently important. There is good reason for believing that the most accurate determinations of the Sun's distance must be made with the help of the minor planets. We have already explained that when the distance of one body in the Solar System from the Earth has been in any manner ascertained, then the distances of all the other bodies belonging to the same system can generally be deduced by a simple calculation. It seems likely that the distance of a minor planet from the Earth can be measured directly with much greater accuracy than can the distance between any other two bodies in the Solar System. It is, therefore, apparent that the application of photography to the detection of additional minor planets should be sedulously cultivated even for this object alone. It is quite probable that among the smaller minor planets there must be many whose distances from the Earth admit of being measured with great precision by the use of photographs obtained nearly simultaneously at observatories in the northern and southern hemispheres respectively.

I have been thus particular in referring to the asteroids, because, as will presently be explained in a chapter devoted to the purpose, they afford the most accurate means that we possess for the determination of the distance of the Sun. But before concluding this sketch, I have also to mention some few other bodies belonging to the Solar System, which are of more or less direct significance in connection with our present subject.

We have first of all to note that several of the planets are attended by satellites, which accompany their primaries in their movements around the Sun. The Earth is thus provided with the Moon, a globe of which the diameter is 2,160 miles, or rather more than a fourth of the diameter of the Earth. The average distance at which the Moon is placed is 238,000 miles, and the period of one revolution round the Earth is 27·322 days. Mars is accompanied by two satellites—Phobos and Deimos, as they are called—the former of which performs a feat unique in the scheme of things celestial by revolving around its primary in a period which is less than one-third of that which Mars himself requires for the completion of one of his own rotations. Jupiter is accompanied by the four satellites which have been known from the earliest invention of the telescope by Galileo. To these a fifth was added on the 9th of September, 1892. It is an object of extreme minuteness, only to be discerned by a highly-skilled observer, furnished with a telescope of power comparable with that of the great Lick instrument in California, by which Prof. E. E. Barnard detected it on the day I have named. Saturn, besides having its system of rings to which we have already referred, is attended by a retinue of no fewer than eight moons, some of which are easily to be discerned, while others are objects of no little difficulty. Uranus, too, is furnished with four such small companions, and Neptune with a single one. But for the several particulars relating to these and the other satellite-systems, reference may be made to the Appendix to this work. Here it only remains to recapitulate one or two circumstances connected with the entire system. The orbits of the planets all lie very nearly in the same plane. This is, at least, true of the principal bodies, but it may be necessary to add that some of the asteroids depart rather widely from that average plane in which all the others revolve. Then, too, the movements of the planets in their orbits are all performed in the same direction; this is a very interesting circumstance, inasmuch as it contributes an essential element to the stability of the Solar System. The orbits of the satellites to each planet, with but a single exception, lie in planes closely adjacent to that plane near which the planets themselves revolve. An exception to the almost universal application of this rule is presented in the case of Uranus, the satellites of which, having evidently been adjusted by some special influence, revolve under conditions quite unlike those to which all the other bodies in the Solar System are submitted. Setting aside such exceptional cases as are here referred to, we may assert that the direction in which the satellites revolve are all identical with that pursued, without exception, by the primaries themselves. I purposely dismiss with a single remark another class of bodies which belong to the Solar System; I allude to the comets, which with their minute associates, the shooting stars, have little further connection with this volume. It is not now necessary to add more than the remark that while comets are known to move in conformity with the laws of gravitation, their orbits are generally highly eccentric, and are often inclined at angles of very considerable amount to the plane in which the planetary motion is performed. The orbits of some of the most important comets, as well as certain orbits followed by meteoric swarms, are represented in the diagrams of the system.

We may think of our Solar System as a group of relatively small bodies revolving around a central Sun. It will be noted that while treating of the bodies in the Solar System, I have as yet said little or nothing as to the stars, properly so called. And this for the excellent reason that the stars have no connection with the Solar System. They are each of them suns, in many cases quite as large and bright as our own Sun, in some cases far larger and brighter. Stars, no doubt, may have systems of planets or other bodies revolving around them in the same manner as our Sun. Of such bodies it is utterly impossible for us to learn anything; for inasmuch as the suns which illuminate them are dwarfed by distance to inconspicuous points of light, we should be totally unable to see any little globes in their vicinity which such suns might illuminate. And as to the distance by which these other systems are separated from that to which we belong, let it now suffice to say that if we were to draw a map of our Solar System in which the Earth was placed at a distance of one inch from the Sun, the nearest of the stars on the same scale would have to be somewhere about a dozen miles away.

CHAPTER II.

THE DISTANCE OF THE SUN.

THERE is no problem of greater importance in the whole of astronomy than that which is to occupy our attention in the present chapter. On the determination of the distance of the Sun depends our knowledge of the magnitude of the great luminary itself, as well as of the features it may present. The Sun's distance forms a necessary element in any conclusions to which we may come as to the dimensions of the Solar spots; the altitude to which a prominence is projected, in the course of a Solar explosion; or the number of miles which are to express the limit of the Solar corona. Our calculations as to the endurance of the Sun's heat must also be based on the distance of the Earth from the Sun, inasmuch as the figures which concern us in this important investigation depend on the size of the Sun, as well as on its mass, and involve elements which cannot be known with accuracy, so long as there is any uncertainty about the distance of the luminary itself. Then, too, as has been already mentioned, the mean distance of the Sun from the Earth is the natural unit of length in which we express the distances of other heavenly bodies. A knowledge of this important constant is required before we can tell the velocities with which the different planets are animated. We can neither measure the diameter of Jupiter, nor the distances of the satellites from the great primary round which they revolve, until we have established the value of the same fundamental unit. Nor is it merely in its innumerable applications within the confines of the Solar System that the determination of the Sun's distance has an importance far transcending that which belongs to any other problem in practical astronomy. In that supreme effort of the surveyor which is involved in the determination of the distance of a fixed star from the Earth, the base line, from which alone the necessary measurements can be effected, is this very Solar distance which is the object of our study in the present chapter. On all these accounts it is therefore desirable to apply the utmost resources of accurate astronomical methods, in the effort to obtain a solution of this problem with whatever precision the circumstances will permit. To set forth this matter fully is therefore an important part of our present work. There is all the more interest in the development of the subject when we find that many different departments of astronomy can be made to contribute towards its successful treatment. Indeed it not unfrequently happens, as we shall presently see, that investigations into astronomical processes which seem at first to lie wholly apart from any question as to the Solar distance, nevertheless often lead to determinations of that distance under circumstances of the highest interest. It is characteristic of the problem that in every case indirect methods have to be resorted to for its solution. There is no means of actually finding the Sun's distance by observations directed to the Sun itself; we have to make observations of various other objects, and then to infer the Sun's distance from a computation often of a very elaborate description. Sometimes, indeed, the distance of the Sun has to be ascertained by the interpretation of certain facts of observation, with the help of a recondite dynamical theory. Indeed the search for the Solar distance by the various processes available, leads to the consideration of many of the most interesting chapters in astronomy.

Among those who have chiefly contributed in later years to the investigation of this important subject, I may specially mention the name of Sir David Gill, his Majesty's Astronomer at the Cape of Good Hope. He has devoted an amount of care to the study of the Solar distance which entitles him to speak on the matter with the very highest authority. One of the best determinations of this important constant that has yet been made is due to his indefatigable zeal. I here refer to his celebrated series of observations on Mars, at Ascension Island, in 1877, of which more must be said later on. But my present object in referring to Sir D. Gill in connection with this subject is to call attention to the valuable series of papers in the "Observatory," in which he has laid down in the most thorough manner the principles on which our determination of the Sun's distance must be ultimately based. He has reviewed and criticised the several methods by which the great problem can be solved. I am glad to acknowledge my indebtedness to these papers in the discussion contained in this chapter and the next.

The first point to be considered is the degree of precision which it might be reasonable to expect in the present state of astronomical observation. It has been shown that the determination of the Sun's distance can be made with such accuracy that we may feel perfectly certain that our results are not erroneous to the extent of one per cent. of the whole. We also feel quite confident that there is not the least prospect of any such improvements being made in practical astronomical work as would enable us to determine the distance of the Sun to within one ten-thousandth part of the whole. Between the two limits I have named lies that degree of precision excluding errors as large as one-thousandth part of the whole. This is the attainable precision for which practical astronomers are now striving. We cannot, indeed, up to the present assert that the Sun's distance has been ascertained with the accuracy which is thus implied, but we know enough to feel confident that the limit thus indicated is one which we may reasonably expect will at some time be attained. We may, then, say that at present the best efforts for the determination of the Sun's distance aim at giving this measurement an accuracy which would make it correct to within one-tenth per cent.

Speaking generally, we may describe the available processes as classified under three different heads. There is, first of all, the theoretical method, in which, from our study of the laws of gravitation, we discover what the distance of the Sun must be, so that the movements which theory attributes to the planets shall be identical with those which observations actually show them to possess. One advantage of such a method of investigation is that it can be based upon a vast number of observations. These observations can be focussed on the inquiry to almost any extent, and in this way a value of the Solar parallax can be deduced, as the result of many years of labour, which may be expected to be more correct than one which has been derived from any observations of some single phenomenon, like the Transit of Venus. To the investigation of this method another chapter will be devoted. There is, secondly, the physical method by which the properties of light are called to our aid. The research thus suggested is of a very instructive kind, inasmuch as it illustrates the way in which different branches of research into the deep things of Nature become interwoven. This method will also require distinct treatment. Finally, there is the surveying method, such as that afforded by the minor planets, in which, by the use of elementary geometrical principles, we solve the problem of the Solar distance. If it be said that this method involves much less imposing conceptions than are required in the other processes, it must be admitted that the logic of the humble process is, perhaps, the most satisfying of all. To their consideration we shall devote the attention which their importance deserves.

Before entering on the details of such methods for determining the distance of the Sun as are afforded by the Transit of Venus, it will be well to explain the general principle underlying them, and thus to emphasise their contrast with the gravitational and physical methods to which we have already referred.

It is the essence of the present method that it depends on a problem of simple surveying; the principles on which the survey is conducted being of like character with those which are employed in making a survey for any terrestrial purpose. To make the method intelligible it must be explained how the problem of measuring the dimensions of our Earth is actually solved, for it is from the measurement of the Earth as a base that the celestial distances are obtained. The first problem is to determine with the utmost refinement of accuracy the length of a definite line suitably chosen on some extensive level surface, such as Salisbury Plain or other similar locality. In the first place, the ground for some miles having been conveniently levelled, the measurement of it is effected by means of bars, which have been carefully compared with a standard of length. Precautions must, of course, be taken to obviate the effect of changes of temperature in altering the lengths of the bars; many ingenious contrivances have been devised for this purpose; one of the most satisfactory being that known as Colby's measuring rod, in which, by employing a composite arrangement of a bar of steel and a bar of brass, the effect of changes of temperature can be compensated. The principle of Colby's rod is exhibited in the adjoining diagram, where the dotted lines signify the positions into which the bars of different metals are brought when an elevation in temperature takes place. The outer bar, which expands most, is made of brass,

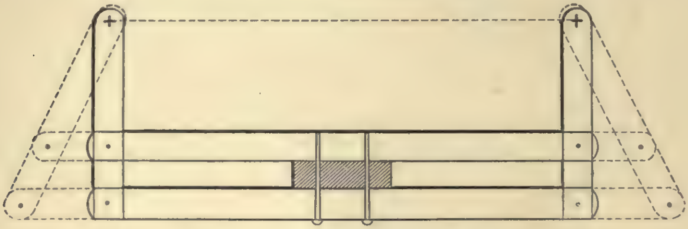


FIG. 7.—COLBY'S MEASURING ROD.

and the other is of steel. At the extremities of the cross-pieces, linked to the ends of the bar, gold plates are inserted, on each of these gold plates a pair of cross lines (+) is ruled, and the adjustments are so made that the distance from the intersection of one pair of these lines to the other shall remain constant notwithstanding the fluctuations of temperature to which the apparatus may be exposed. Each of the Colby rods is ten feet long, and in order to place them one after the other for the purpose of the measurement, an arrangement has to be provided by which the distance between the cross lines on the one rod and those on the rod next adjacent shall admit of being measured with the same accuracy as the distance between the points themselves on the same rod. This is effected by a pair of microscopes with parallel axes rigidly bound together. The distance between the foci of the two microscopes having been determined by independent investigation, the extremities of the two adjacent rods are brought into the corresponding foci of the microscopes. In this way it is ensured that the distance between two consecutive rods has a determined value, and thus the whole length of the base line can be correctly ascertained. To illustrate the accuracy with which this task of measuring the length of the base line has been accomplished, it may be mentioned that on a re-measurement of a distance of about five miles, the difference between the distance thus found and that previously obtained only amounted to about a couple of inches.

Such is the first step in the problem of determining the length of a degree on the meridian of the Earth. It will presently appear how necessary it has been to lavish all the care, of which we have given an indication, on the exact determination of the base line. Any error by which the measurement of the length of the base line was originally contaminated would propagate itself in every subsequent determination, and its effect would be increasingly disastrous with every increase in the scale of the operations. To ensure that we shall be right about the miles in the final operations it is necessary to take care that we are right about the inches in the initial stages of the undertaking. Let us, then, suppose that a base line, some few miles in length, has been ascertained with a precision that leaves nothing to be desired. The next step in the process is to secure an object suitable for the vertex of a triangle, of which the two extremities of the measured line form the base. It would be proper for this vertex to lie at a considerable distance, say about three or four times the length of the base. To indicate the vertex of the triangle a definite mark is required, and in the actual conduct of the great surveying operations, one of the most successful marks was obtained by a piece of lime heated to incandescence by the oxy-hydrogen flame. At night this admitted of being observed and measured with a very high degree of precision. Let us suppose that at the extremities of our base line telescopes are placed which are provided with divided circles graduated into degrees, minutes, and seconds. The observer, taking his station at one end of the base line, observes the angle subtended at his instrument by the incandescent lime at the vertex, and a like mark at the other end of the base line. In fact, what he does is simply to measure one of the angles at the base of the triangle. In like manner, an observer stationed at the other end of the base line, measures with his instrument the angle at the corner of the triangle where he is stationed. These operations being completed, a trigonometrical calculation is necessary. The length of the base line is known, and the two angles at that base have been measured. From these data it becomes possible to calculate the sides and remaining angles of the triangle. Here, then, we see that the only measurement of length required is that of the original base line; the lengths of the sides of the triangle being then determined from the measurements of the angles. Each of the sides of the triangle thus ascertained may in like manner serve as a base whereupon a new triangle is to be erected and measured, and the operation may be repeated indefinitely. At each step in the process the sides of the triangles employed become greater and greater until, at last, we are able to form a triangle so great that the base shall lie, let us say in Wales, and the vertex be at the summit of a mountain in Ireland. The sides of such a triangle have, indeed, been successfully measured when upwards of one hundred miles in length. Thus a network of triangles has

FIG. 8.—HOW TO CONDUCT A SURVEY.

been spread all over a country, and, indeed, over a continent, and a striking verification of the precision with which the measurements have been made is incidentally afforded in the course of the work. For, besides the original base, other subsidiary bases have been measured in localities hundreds of miles distant. Thus a base line was measured on the shores of Lough Foyle, in the North of Ireland, and when the survey, based on the measurement of arcs in England, had extended across the Channel, it was possible to compute the length of the Lough Foyle base. It is interesting to note that the computed length only differed by a few inches from the measured length, the whole base being about five miles long. By operations of this kind, extended over large regions of the Earth, we have been enabled to determine the size of our globe.

Let us, first, suppose the Earth to be a perfect sphere; of course, as a matter of fact, this is not the case; but for the present it will be convenient to make the assumption, and we can afterwards introduce the necessary correction. Draw, then, any meridian on our globe, and mark on that meridian two points, whose distance shall be the object of special examination. The measurement of the difference in latitude between these two points can be effected with wonderful accuracy by the instruments of the astronomer. It is a well-known theorem that the latitude of any place is equal to the altitude of the Pole above the horizon. This circumstance enables the latitude to be measured, for though the Pole is not a point marked by the presence of any star, yet by observing the Pole-star as it passes above the Pole, and again as it passes below the Pole, in the course of the diurnal rotation, the actual altitude of the Pole above the horizon can be obtained with all needful precision. So great is the accuracy attainable in the application of this process with our modern instruments, that the astronomer can note a difference between the latitude of the north wall of his observing room and that of the south wall. In fact, just at the time that these lines are being written, there is no little stir in the astronomical world arising from the fact that the North Pole appears to have a motion in the Earth by which it is sometimes fifty or sixty feet nearer to Europe than it is at other times. This may seem an extremely small matter, considering that the observatories are thousands of miles from the Pole. It will, however, indicate the refinement of which the determinations of latitudes are susceptible, to reflect that it is by observations of the variations of latitude alone that this extremely slight movement of the North Pole has been brought to light.

From these considerations it will be obvious that there is no practical uncertainty attending the operation of putting two posts into the Earth on the same meridian at a distance of exactly one degree apart. A very few feet is the outside limit of possible error. This having been done we can now, by our surveying operations, determine the number of miles and fractional parts of a mile between these two posts. In this there is also, as we have seen, but little practical uncertainty. The error in the base line, to begin with, could not have been more than an inch or two; although it would, of course, be somewhat magnified in the measurement of this long line of sixty-nine miles, which corresponds to one degree. We may, however, feel certain that the whole length has been measured rightly to within a few feet, and as we know also that the posts are one degree apart it follows that we have determined the length of a degree with a precision which is certainly right to within a thousandth part of its total amount. The circuit of the Earth itself is hence derived by a very simple calculation; there are, of course, 360 degrees in its circumference, and therefore by multiplying the number of miles and fractional parts in one degree by 360 we obtain the measurement required.

Before going further I may mention how it is that by surveying work of this description we show that the Earth is not a perfect sphere, and determine in fact the actual extent of its protuberance at the Equator. If the Earth were a sphere it would necessarily follow that the length of a degree should always have the same value, whatever were the part of the globe on which the degree was measured. As a matter of fact, however, we do not find that the length of a degree is the same when measured on the meridian in considerably northern latitudes, and when taken on the Equator. It becomes decidedly longer as we approach to the Pole. This of course implies that the curvature of the Earth near the Equator is more rapid than it is near the Pole. In other words, that the Earth is flattened at the Pole. By the discussion of the actual lengths which have been been obtained from arcs of the meridian when measured in different latitudes, it has thus been possible to ascertain the exact shape of the Earth with all needful precision. The Equatorial semiaxis of the Earth is 20,926,202 feet, and the Polar semiaxis is 20,854,895 feet.

We have now to consider how these operations of surveying are to be conducted in their application to objects not on the Earth's surface, and we shall learn that the determination of the distance of the Sun, or even of the stars, is effected by measurements ultimately founded on the investigation of the length of the terrestrial base line in the manner already described. Suppose that two observers agree to observe some celestial object from points on the Earth, as widely separated as may be convenient; since we know the dimensions of the Earth and the localities of the observers, we know the distance by which they are separated. If they are able to measure the angles at the base of the triangle whose vertex is the celestial body, and of which they occupy the two corners, they will then have the means of determining the distance of that body, just in the same way as the distance of a definite mark on one of the Mourne Mountains in the East of Ireland was determined from observations in Wales at the extremity of a line of known length. But the peculiar difficulty in the extension of this method to the investigation of astronomical distances arises from the tremendous magnitude which those distances generally attain. Not, indeed, so much from the absolute magnitude of the distances, as from the circumstance that the length of the base available to us bears such an extremely small proportion to the distances that are sought. To illustrate one peculiar difficulty in determining the distance of the Sun, I will suppose that two observers take the greatest possible base line that they can obtain on this Earth. I will place them in imagination at the extremities of an equatorial diameter 8,000 miles apart. That is the widest interval that can possibly separate them, but it is so small in proportion to the Sun's distance, that the minutest error in the measurement of the angles, on the determination of which the process depends, will introduce into the result an uncertainty of monstrous proportions. We may realise the shape of this ill-conditioned triangle by representing it on a small scale. Suppose it were drawn with a base of one foot; each of the sides of that triangle would have to be more than two miles long. The awkwardness of the figure may be exhibited in another way, for an observer on the Sun would find the Earth appear to him to be about as large as a tennis ball would look at a distance of half a mile. It is, therefore, impossible to avoid errors very significant in comparison with the total quantity to be measured, when we are constrained to work with triangles so badly shaped as those which Nature requires us to use. If we could find an available triangle of which the base was 800,000 miles instead of 8,000 miles, then the determination of the Sun's distance would be attended by no great uncertainty. But our appliances for measuring angles, perfect though they may be, still cannot deal with quantities which only amount to a very minute fraction of a second. The diameter of a man's head, if it could be seen at the other side of the Atlantic, would subtend an angle of about the hundredth part of a second. Need it be said that such an angle is of a minuteness almost imperceptible to any ordinary measurements. It would seem almost impossible to avoid making errors as great as this, and even far greater, with the most refined appliances ever devised for angular measurement. But such minute errors would introduce tremendous errors into the determination of the Sun's distance, if we depended merely on one of these excessively long triangles. We are, therefore, compelled to resort to indirect methods by which greater accuracy can be obtained.

It is, however, quite true that whatever instrumental process we employ, our work must be ultimately founded on a base of known length and angles measured from its extremities; for though the various processes we shall have to refer to may appear to depend on other principles, yet in reality they all ultimately resolve themselves into this fundamental investigation of trigonometry. The simplest of all the instrumental methods, certainly in so far as its principle is concerned, is that which involves the measurement of the distance of the planet Mars or of one of the minor planets, which can be made to answer the same purpose. We have already explained that if any one of the distances in the Solar System be determined, then every other distance will follow easily. The relative proportions of the different distances being known by independent investigations, it is only necessary to discover the absolute value of a single one in order to determine all with accuracy.

We shall suppose that two astronomers, whose observatories are stationed on the same meridian, agree to observe a celestial object belonging to the Solar System, to determine its distance. We shall further assume that a minor planet has been chosen for this purpose, because, from the small size of the object it forms an extremely definite point, stellar in its minuteness, and admitting of highly accurate micrometric measurements from surrounding stars. We shall further suppose that the observations are to be made simultaneously, though this is a condition that can hardly be realised in practice. As, however, the planet is in motion, it will either be necessary that the observations at the two stations be made at identical moments, or else that the means be at hand of correcting the observations for the effect of the movement of the planet in the interval which elapses between the observation of the astronomer in one hemisphere and that made by his fellow-worker in the other. As a matter of fact, the observations never can be made quite simultaneously; they will differ by minutes, or more likely by hours or days. The method of correcting for errors thus arising is now, however, so well understood that it involves but little uncertainty, and we may, therefore, for the purpose of our present investigation, suppose that they have been made with absolute simultaneity. By preconcerted arrangement between the two observers, certain stars have been chosen which lie in the immediate vicinity of the planet's track, and the quantity to be measured is the angle which the planet makes day by day with the selected star lying nearest to it. And since each of the stars is situated at a distance from us so vast that for our present purposes it may be regarded as infinite, the direction in which it is seen by each of the two observers is practically identical. The diameter of the Earth is quite an inappreciable magnitude in comparison with a stellar distance. The angle between any two of the stars, as measured from one of the observatories, would thus appear precisely equal to the angle between them as measured from the other. Take, for instance, such a pair of stars as Castor and Pollux, whose angular distance is a certain number of degrees, minutes, and seconds. Now, no matter at what point of the globe the observer might be stationed, the angular distance of Castor and Pollux would always appear to be the same, in so far at least as any possible effect due to the observer's change of position was concerned. If, however, a planet belonging to our system were substituted for Castor, then the angular distance between it and Pollux would be no longer the same when measured from different parts of the Earth. It is true that the lines drawn from various points on the Earth to Pollux would all appear sensibly parallel, but the lines drawn from observers in different localities to the planet would not all be parallel, inasmuch as the dimensions of our globe would bear a measurable, though no doubt a small, ratio to the actual distance of the planet. By observing the alteration in the angular interval as viewed from different points on the Earth, we are then enabled to calculate what the proportion must be between the distance by which the observers are separated and the remoteness of the planet from both of them. In the actual conduct of the operations, stars are chosen which are apparently in closer contiguity to the planet than Castor to Pollux in the illustration I have taken. In fact, the closer the bodies seem to lie on the surface of the heavens, the more accurately can the measuring operations be conducted.

I will suppose, for convenience, that the planet happens to lie in the Equator. We shall also assume that the two observatories are on the same meridian, at equal distances north and south of the Equator, and that the actual distance by which the astronomers are separated is, let us say, 2,000 miles; I do not mean 2,000 miles around the curved surface of the Earth, but right through the Earth from one observer to the other, in the way represented in Fig. 9 (p. 42). Let S be the planet, A and B the places of the two astronomers; then A S is the direction in which the northern astronomer at A sees the planet, and B S the direction in which it appears to the southern astronomer at B. The immediate object of the research is to determine the angle between these two directions with the utmost degree of accuracy. In other words, what we desire to find is the angle A S B.

Suppose that the man at A measures the distance between the planet and a star on the same meridian as presented from his point of view, and that the man at B measures at the same moment the angular distance between the planet and the same star; we then know that the angle A S B must be the difference between the two values. Thus the observation of the apparent angular distance between the planet and a star conveys the information we require. The suitability of this method for the determination of planetary distances will be obvious if we consider the advantageous circumstances under which such measurements can be made. There is no kind of astronomical magnitude which admits of being measured with greater precision than the angular distance between two star-like points of adequate brightness, when those points are sufficiently close together to be visible in the same telescopic field of view. The instrument employed for observations of this class varies under different circumstances, but in the majority of cases the filar-micrometer is found to be specially applicable. The apparent distance of the two objects is indicated by the filar-micrometer in terms of the revolution of a screw, by which one of the spider lines with which the instrument is provided is carried from one of the stars to the other. The construction of this most useful appliance in the observatory will be readily understood. There are, generally speaking, two parallel movable lines, and a pair of adjacent fixed lines at right angles thereto; when in use one of the fixed spider lines is first carefully adjusted so as to pass through the two points whose distance is to be measured. Each movable wire is then placed on one of the stars, and the corresponding readings of the screws are recorded; the two spider lines are then crossed and adjusted so that the stars are interchanged, when the readings of the screw are again taken. By this process we can measure with great accuracy the angular distance of two celestial bodies. In the first place, the objects are so close together as to obviate various instrumental sources of uncertainty. The effects of refraction in altering the apparent places of the stars are so nearly equal, in the case of objects closely adjacent, that the small outstanding difference can be allowed for without any uncertainty. Another advantage of this process is that it admits of being repeated many times —indeed, quite as often as may be desired; for the planet will generally remain sufficiently near to the Earth to be available for observation during several consecutive weeks. It is thus obvious that from the mean of the several results which there is an opportunity of accumulating, a much more accurate result may be expected than when the required measurement only depends on the observations of a single phenomenon, such as the Transit of Venus. It should also be mentioned that there is considerable latitude as to the choice of the minor planets which may be utilised for this purpose. It is, of course, especially desirable that the body employed should be one which comes comparatively near to the Earth—that is to say, whose perihelion distance is as small as possible. It will naturally not often happen that a planet comes into opposition at the same time as it is in perihelion, but there are so many minor planets available that the desired conditions can frequently be realised in one or other of them. A considerable proportion of their number, especially among those most recently discovered, would, however, be unsuitable for the operation, on account of their excessive faintness; the measurements losing in accuracy when either of the points to be measured is so dim that it cannot be seen in a field in which the lines of the micrometer are represented as black lines on a bright background.

It will also be obvious that a similar method is applicable to Mars, and measurements have been made with some success in which this planet has been employed. Indeed, Mars would seem, in one way, to be eminently suitable for this purpose, on account of its proximity, when it happens that the time of opposition coincides with that of the passage of the planet through perihelion; on such an occasion the distance between Mars and the Earth is but little more than one-third of the distance from the Earth to the Sun, and consequently there is a considerable displacement of Mars relatively to the stars in its vicinity. In the autumn of 1892 Mars approached the Earth to within a distance of 35 million miles. The diameter of our globe subtends at the Sun an angle of about 17½ seconds; an observer placed on Mars on such an occasion would, therefore, see the diameter of the Earth subtend an angle of about 46 seconds. So, conversely, observers at opposite ends of a diameter of the Earth would see Mars at points on the sky which were as much as 46 seconds apart. This amount could not, however, be fully realised, for it would be impossible to observe from two such stations; but a considerable part of it can, and it may therefore happen that by suitable arrangements the apparent distances between Mars and adjacent stars will differ by from 20 to 30 seconds, as viewed from the two co-operating observatories. If this distance could be measured accurately to within the fiftieth part of a second, we could then determine the Sun's distance with a degree of precision which would leave nothing more to be desired. The difficulty in the successful application of the process arises from the fact that Mars, at the time of such an observation, shows a large and brilliant disk, and its margin is not defined with sufficient precision to enable the observations to be made with the delicacy which would be necessary. It is a far less satisfactory object for measuring with the spider line micrometer than one of the little planets; and hence it happens that even though the little planets may not approach quite so closely, and consequently the available angle may not be quite so large, yet a result of superior precision can be obtained from the small objects than is possible in the case of the nearer and more brilliant orb.

There is, however, another method of a very interesting kind by which the determination of the distance of a planet can be effected. We have described how such an operation can be conducted by two observers, if, by preconcerted arrangement, at widely different parts of the Earth they observe the same objects as nearly simultaneously as possible. The method is, however, open to the objection that we have to compare the observations made by one observer with those made by another. They often may employ instruments differing in character, and in any case what is known as "personal equation" has to be reckoned with, this being an idiosyncracy, in consequence of which each individual is in the habit of estimating magnitudes in a fashion peculiar to himself. If one man were accustomed to rate angular distances somewhat differently from another—and even the most excellent observers have peculiarities of this description—then it is obvious that when we deduce the distance of the planet from the difference of the observations made by them, there is risk of considerable error. Suppose, for example, that the difference in question is ten seconds, and that, notwithstanding all the precautions taken, one of the observers has measured the magnitude at one-tenth of a second larger than it would have been estimated by the other, then it is plain that the resulting difference must be affected by this error of one-tenth of a second. This is one-hundredth part of the total amount, and the concluded distance from the Earth to the Sun would necessarily be erroneous in the same proportion. In other words, we should have an error of one per cent. in the result. Now we have already laid down that in the determination of this great constant we may reasonably expect to learn the result accurately to within one-thousandth part of its total amount; hence it follows that personal equation, to mention no other source of error, would present a great difficulty in the attainment of the desired standard of excellence. No doubt by a sufficient accumulation of observations, and, still better, wherever it is possible, by the interchange of observers between the two stations, it might be practicable to reduce the effects of personal equation considerably, if not to eliminate them entirely. But it is evident that the particular source of error that we are now referring to would be obviated if a single observer could suddenly transport himself, instrument and all, from one station to the other, so that the two observations would be made under precisely similar circumstances. The individual peculiarities would then disappear.

It so happens that modern astronomers have been able, in some degree, to utilise the principle here suggested, for an observer can avail himself of the rotation of the Earth to convey his observatory thousands of miles in the course of a few hours. Imagine that you are standing on Mars and looking towards our globe, just on the western edge of which an observatory was placed; then, of course, the terrestrial movement of rotation would gradually convey this observatory to the eastern side of the Earth. In that operation, of course, the astronomer would have shifted his position by some thousands of miles. If he therefore looked at Mars, at the beginning and the end of the movement indicated, he will have seen it from such different points of view that Mars will seem to have altered its apparent place on the heavens. This is the process which Sir D. Gill applied with so much success in his memorable expedition to Ascension Island in 1877. The planet in that year approached the Earth almost to the utmost limit of its possible proximity, and Sir D. Gill's scheme involved, first of all, the selection of a site from which the peculiar observations required could be made with the greatest advantage. It is plain that some point near the Equator would be more suitable for this purpose than a station with any considerable north or south latitude. For evidently the displacement of the astronomer at an equatorial station by the Earth's rotation attains a greater value than elsewhere. If Mars remained fixed in the sky the discussion of the observations would be of a comparatively simple character; provided that the planet were observed as soon as possible after it had risen, and then again just before setting. Indeed, if the planet lay in the celestial Equator, then it is easy to show that the apparent shift of the position of the planet on the heavens would be equal to the angular diameter of the Earth when viewed from the planet itself.

This will be obvious from Fig. 9, in which the plane of the paper is now to be taken as the plane of the Equator. The North Pole is at the centre, and S is the place of the planet, but of course the nearness of the latter is enormously exaggerated. From one place of observation the planet is seen in the direction A S, and from the other it appears in the direction B S, the angular difference between those two directions being precisely the angle A S B—that is to say, the angular distance of the stations as viewed from the planet. To determine this angle, measurements have to be made from each station, between Mars and the stars which lie near it; in fact, if we take as a simple instance the case of a star happening to lie exactly in the Equator, it could be demonstrated that the angle of which we are in search may be found as follows: measure the angle subtended by Mars and the star when viewed soon after they have risen; repeat the operation when the objects are nearly setting, then the difference between the two angles so obtained is the angle required.

This method has the great advantage of securing that the same instrument and the same observer shall be employed in both observations. So far, therefore, as the personal equation of the astronomer is concerned a great point has been gained in the adoption of this process; on the other hand, it has one distinct drawback. We have supposed in our description that the planet retained the same place on the heavens during the interval between the two observations; of course this is not the case. The observations are necessarily many hours apart; it is, therefore, obvious that there is a considerable displacement of the planet in the interval, due to its own motion. The change that is actually observed in the angle between the planet and the star must therefore arise from two sources; it is partly an apparent

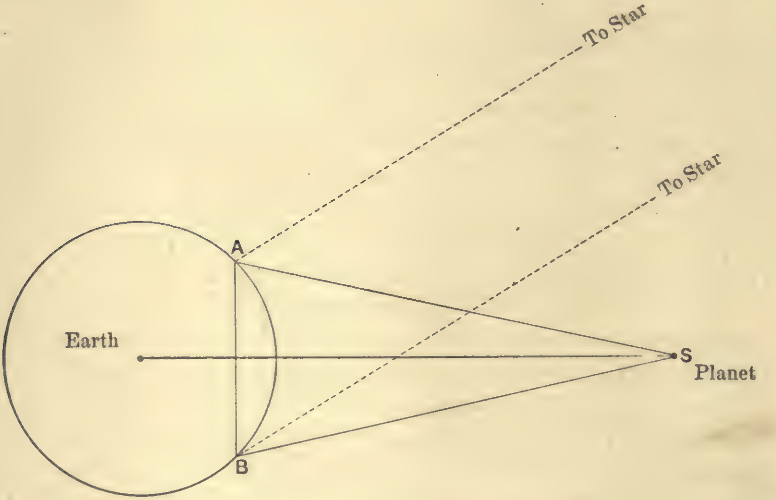


FIG. 9.—MEASUREMENT OF A PLANET'S DISTANCE.

change due to the observer's change of place; it is partly a real change attributable to the actual planetary motion; it is the former element we want, and the latter it is of course necessary to remove. Unless this separation can be effected with extreme accuracy, there is a residuum of uncertainty left in the determination of the apparent displacement which greatly detracts from the utility of this scheme for finding the solar distance. In the other method of observing Mars, by two observers in opposite hemispheres, this difficulty does not arise; for the observations could be made within an hour or so of each other, and by a judicious arrangement of the work the effects of mere planetary movement could be eliminated satisfactorily.

By discussing his entire series of observations made at Ascension, Sir D. Gill was, however, enabled to obviate the difficulties arising from the planetary motion, and thus to obtain a value of the solar distance which will probably rank among the most perfect determinations that we possess of this great constant. I should, however, mention that neither from Ascension, nor from any other station, would it be possible to utilise the whole diameter of the Earth, as the figure might seem to suggest. It need hardly be said that it would be impracticable to observe the planet immediately after rising, or immediately before setting. It must in each case have attained a considerable altitude above the horizon, before atmospheric opacity and refraction would admit of proper measurements being made. This reduces, of course, the extent to which the displacement of the observer affects the apparent position of the planet, and diminishes in a corresponding degree the available angle from the measurement of which the required result has to be found. Obviously, however, the same atmospheric difficulty will prevent the employment of two observatories for the meridian method if they be situated so far apart that Mars would be too near the horizon at either station for the purpose of quite reliable observations. I may also mention that the process just described is as applicable to a minor planet as to Mars. Sir D. Gill, has, indeed, made valuable researches in this direction. It appears probable that these little bodies, when suitable, both as to distance and as to dimensions, for the application of this process, afford the most accurate means known to us of determining the Sun's distance. There can hardly be a doubt that its ultimately adopted value must be mainly dependent upon minor planet observations, made from an Equatorial observatory where the rotation of the Earth gives the means of obtaining the necessary change of site.

CHAPTER III.

THE TRANSIT OF VENUS.

THE most famous method of solving the problem as to the scale of the Solar System is undoubtedly that afforded by the Transit of Venus, though this is not now thought to be so trustworthy as some of the modern processes. Viewed from the Earth, Venus appears to perform its revolution round the Sun in 584 days; but this value of the period is affected by the motion of the Earth. The true period, as it would be seen from the Sun, is 224·7 days. If the orbit of Venus lay in the plane of the Earth's orbit, then Venus would pass between the Earth and the Sun once every 584 days, and the phenomenon of its transit would each time be observed. The orbit of the planet is, however, inclined to the plane of the ecliptic, so that Venus, when in conjunction, generally passes either above or below the Sun, and only on extremely rare occasions is it actually seen to pass in front of the Sun. The line in which the plane of the orbit of Venus passes through the ecliptic, crosses the Earth's track at the positions occupied by the Earth on June 6th and December 7th. If it happened that Venus came into conjunction on, or very near, either of the dates I have named, then a Transit of Venus would be observed. Conjunctions generally occur at dates other than those I have mentioned, and consequently the planet is generally either too high above or too far below the Earth's orbit to permit a transit to take place. There is a remarkable law which connects together the sequence of the Transits of Venus. It so happens that eight revolutions of the Earth around the Sun occupy almost exactly the same time as thirteen revolutions of Venus. If, therefore, the Earth and the planet should be in line with the Sun at any particular time, then in exactly eight years more, the Earth having in that time made eight revolutions, and Venus having made thirteen, the line joining two bodies will again be pointed directly towards the Sun. Thus the occurrence of a transit is generally followed by a repetition of the phenomenon after an interval of eight years. It might naturally be thought that if this law were rigorously true we should witness a Transit of Venus every eight years, but the fact is, that the law is not rigorously true. It is nearly, but not absolutely, accurate to say that Venus accomplishes thirteen revolutions in eight years. As the successive periods of eight years come round, the line joining the two bodies travels slowly round the ecliptic, so that though on two consecutive occasions this line may enter the Sun's disc, yet, when the next similar period has elapsed, the line will have passed away from the Sun and a transit will not take place. Thus we had a transit in December, 1874, and this was followed by another in December, 1882, but in December, 1890, though no doubt Venus was then very close to the Sun, yet the planet was not in front of the Sun, and no transit could be seen.

The first occasion on which the attention of astronomers was directed to the phenomenon of the Transit of Venus, was in the year 1631. Kepler had announced that in the early part of December in that year Venus would be presented in the entirely novel aspect of a black spot in front of the Sun. There can be no doubt that the transit took place, although no one saw it. Gassendi maintained a careful watch on the 4th, on the 5th, 6th, and 7th of December, but without result. In those days the movements of the planets were not known with anything like the precision that is now attained. We can tell to the very minute when such a phenomenon is to take place; but an uncertainty to the extent of hours, or even of whole days, is not to be wondered at in so early a stage of astronomical knowledge. We can thus calculate the precise time of this transit for which Gassendi sought in vain; we find that it took place, while the Sun was below his horizon, between the 6th and 7th December, 1631. The transit occurred, in fact, when Gassendi was altogether at the wrong side of the Earth, so there is no wonder that his desires were frustrated. In conformity with the eight years' period, to which we have already referred, the transit of 1631 was followed by another in 1639; this was the first phenomenon of the kind which was ever observed, nor was it seen by more than two individuals. A gifted young astronomer, named Horrocks, a Lancashire clergyman, had made some calculations on his own account with regard to Venus, and discovered a fact which had escaped the notice of every other man of science, namely, that the planet would be again in transit in December, 1639. Horrocks accordingly prepared to observe the phenomenon with such resources as he could command, and he communicated his prediction to his friend Crabtree, who also made his modest preparations for the observation. It so happened that the day to which the figures of Horrocks pointed was a Sunday, so that the astronomical divine was obliged to sub-divide the day as best he could, between the several ministrations in his church, which he was bound to perform, and the close scrutiny which he desired to maintain of the Sun. It was not, however, till late in the afternoon, when the Sun was about to set, that the clouds, which had for some time hung over the luminary, dispersed, and disclosed to the ardent astronomer the earliest glimpse of a Transit of Venus which any human eye had ever beheld. Crabtree was also successful in seeing the same phenomenon, and visitors to Manchester will do well to look at the fresco in the Town Hall, commemorating in a striking picture his observation of the Transit of Venus.

It does not, however, seem to have occurred to Horrocks or to Crabtree, that the Transit of Venus was destined in future years to attain a wholly exceptional importance among astronomical phenomena, in consequence of the prospect which it offered of the determination of that great constant in astronomy, the Sun's distance. It was nearly a hundred years later when the illustrious Halley pointed out that, by observations of this phenomenon, the distance of the Sun from the Earth could be determined with a far higher accuracy than had been before obtained. Nor is there an element of pathos wanting in the story of Halley's connection with this problem. The great astronomer, though he knew he should never participate in the observation of the transit himself, yet carefully laid down the principles by which the next generation of astronomers were to be guided. Halley was sixty years old when he commended the problem to the notice of the Royal Society of London. He said that though the next Transit of Venus could not occur for forty-five years, yet that he desired to urge the importance of the phenomenon on the astronomers who might undertake the work, and he adds: "I earnestly wish them all imaginable success in the first place, that they may not, by the unseasonable obscurity of a cloudy sky, be deprived of this most desirable sight, and then, that, having ascertained with more exactness the magnitudes

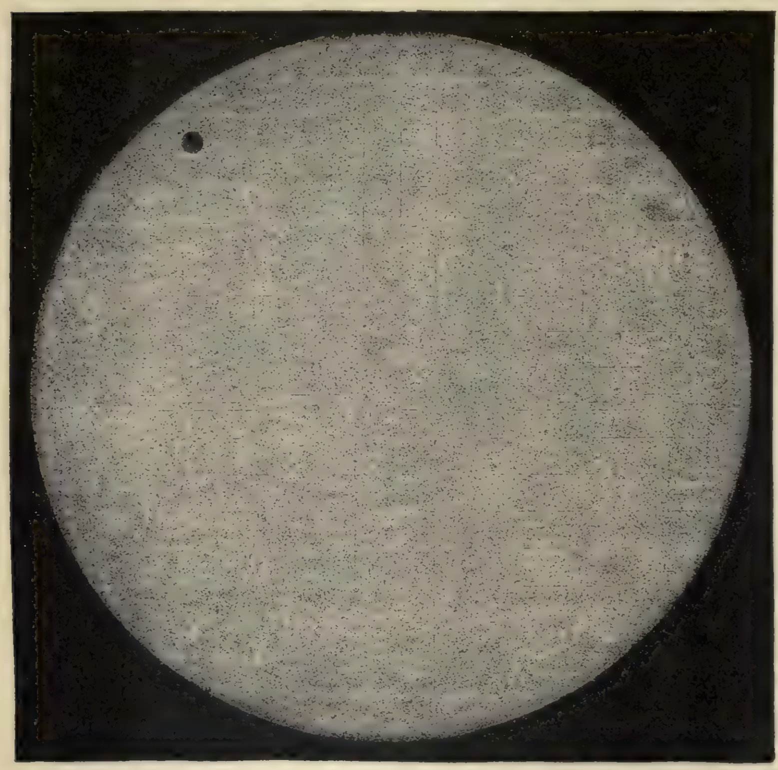


FIG. 10.—VENUS ON THE SUN IN THE TRANSIT OF 1874.

of the planetary orbits, it may redound to their immortal fame and glory."

The apparent size of Venus varies greatly, in accordance with the varying distance between it and the Earth. In Fig. 4 a series of views are given, exhibiting the relative sizes of the planet at its different phases. At the time of transit the planet is at its nearest point to the Earth, and therefore it shows a larger disc than at other times. The appearance of the planet in the act of transit is shown in Fig. 10, which reproduces a photograph taken in 1874.

In one respect the use of the Transit of Venus may be compared with the processes already described, in which we do not attempt to discover the distance of the Sun directly—what we aim at is to discover the distance of the planet, and this is achieved by observing the shift of the place of the planet when viewed from stations on the Earth separated from each other by a distance as wide as can be attained. We ascertained the shift of the apparent place of Mars by comparing the planet with the stars in its background; now, too, we discover the alteration in the apparent place of Venus by careful measurement of the planet as seen upon the Sun from different parts of the Earth. There can be no doubt that if it were possible to measure the apparent distances of the planet from various stars at the time Venus was as near to us as she is at the moment of transit, then the Venus method would give a far more satisfactory result than any other method of finding the Sun's distance. But the fact that Venus moves in an orbit inside that in which the Earth revolves makes all the difference between the case of Venus and that of an exterior planet. When Mars comes nearest to our globe we are situated with the planet on the one side of us and the Sun on the other, and therefore the planet is on the meridian at midnight, when it is seen under the most advantageous circumstances for the measurements that are wanted. But when Venus is nearest to the Earth it reaches the meridian at noon-day, and hence, of course, there is not the slightest possibility of observing stars in its neighbourhood. No doubt bright stars can be seen in the daytime with the telescope, but this is only practicable when such stars are at a much greater distance from the Sun than Venus would be on such an occasion. We never can employ stars as points of reference from which to measure the distance of Venus in any investigation of the Sun's distance. The Sun itself, however, serves the same purpose on such rare occasions as are afforded by the Transit of Venus. It should, however, be observed that the apparent shift in the situation of the planet, as projected on the Solar disc, by the change in place of the observer, is not so great as would be the shift of Venus with reference to the stars. The Sun itself, though further off than Venus, is still at a distance so far comparable therewith that its apparent place on the heavens is appreciably changed with the alteration in the locality of the observer. It is, therefore, only the difference between the displacement of Venus and the displacement of the Sun that is available for the purposes of our measurement. Suppose that from two stations, as far apart as possible in that hemisphere of the Earth which is

FIG. 11.—THE PATHS OF VENUS ACROSS THE SUN IN THE TRANSITS OF 1874 AND 1882.

turned towards the Sun, photographs were taken simultaneously at a time when Venus was in transit. Then the place of Venus on the Sun's disc, as shown in the photographs—that is to say, the distance of the planet from the Sun's limb and the direction of the line joining the centre of Venus to the centre of the Sun—would differ in the two photographs. The difference could not be attributed to any actual change in the position of the planet, inasmuch as we have assumed that the photographs were taken simultaneously; it simply arises from the circumstance that the photographs were taken from different localities on the Earth. By accurate measurements made upon them it thus becomes possible to determine the shift of the planet relatively to the Sun. As, however, the relative distances of Venus and the Sun are known, there is no difficulty in calculating further the apparent displacement of the planet on the sky, due to the fact that the photographs were taken from two stations at a known distance on the surface of the Earth. This affords a determination of the distance of the planet, the shift of which on the sky represents the angle subtended by the two terrestrial stations when viewed from the planet, as we have already seen in this and the preceding chapters. Thus, as already explained, in the case of the planet Mars, we have the means of deducing the value of the Sun's distance. Unfortunately, considerable difficulties arise in the practical application of this method. In the first place, the photographic images of the celestial bodies are often lacking in the delicacy and precision of outline that would be necessary to render them available for such refined investigations. The Sun's limb often, to the observer, appears as if "boiling," and the agitation to which this word corresponds affects photographs of the limb to a degree very prejudicial to exact measurement. Owing to the rarity of the Transit of Venus—in fact, no further phenomenon of this description can take place till the year 2004—it is vain to expect to eliminate by the accumulation of observations the troubles by which they are injuriously affected. Hence, no very accurate evaluation of the important constant has been derived from measurements of photographs of Venus in transit across the Sun's disc.

But there are other ways in which the phenomenon of the

Transit of Venus can be applied to ascertain the Sun's distance. Indeed, Halley's suggestions as to the use of the transit for this purpose were made long before photographic processes were thought of. It will be remembered that the essential element to be discovered is the shift in the apparent position of the planet as presented to observers from widely different parts of the Earth. Halley thought that the magnitude of this shift admitted of being determined in a very unambiguous manner. In the course of its transit Venus is first detected like a dark bite taken from the brilliant margin of the Sun. After a few minutes the planet enters entirely on the bright solar disc, moves across the luminary until it touches the other side, and then, a few minutes later, the disc of the planet has completely passed off the bright surface, and the phenomenon of the transit is at an end. The whole time of transit varies according to circumstances, but we may mention four hours as being, perhaps, about an average interval. It is quite clear that owing to the shift in the apparent place of the planet on the disc of the Sun, dependent on the locality of the observer, the line traversed by the planet in moving across the solar disc must in general be different for two observers located at distant parts of the Earth. For our present object such sites are suitably chosen when the difference between the lengths of the two journeys of the planet is the greatest possible. If there were any way by which the length of the chord traversed by the planet could be measured with sufficient accuracy, then the positions of the two chords on the Sun's disc, as observed from the different stations, would be determined. The interval between the two chords could then be learned, and thus the shift of Venus on the Sun, which is the magnitude we want to find, would be determined. It occurred to Halley that the required lengths of these chords admitted of being determined with all needful precision by the simple operation of noting from each station the time which Venus required for the act of transit. When the planet is invading the Sun's limb, and when the black spot has completely entered on the bright disc, though still in contact with the limb, the phase of First Internal Contact is said to be reached. So, too, as the planet touches the limb of the Sun, after it has well-nigh completed its journey, Second Internal Contact is said to be reached. For the sake of accuracy in specifying the moment at which the transit commences or ends, the phase known as Internal Contact is chosen. This phase admits of much more exact observation than the External Contact, for it is not easy to mark, with the required precision, the definite moment when the black disc first commences its entry, or the definite moment when it at last takes its leave.

It is, therefore, on the Internal Contacts that reliance has to be chiefly placed in the observations of the Transit of Venus for the determination of the Sun's distance. The time occupied by the planet in passing across the Sun from one Internal Contact to another depends, of course, on the situation of the observer. From one properly chosen station on the Earth the duration of the transit will seem less than it does from any other point of the globe. In like manner a site can be fixed upon from which the observer will find that the transit occupies a longer time than it does from any other part of the Earth; the greatest possible difference in the length of the transit, as observed from two terrestrial stations, being about twenty-five minutes. It should, however, be remarked that this would only be attainable at points on the Earth's surface ideally fitted for the purpose. Astronomers have in practice to be contented with observations obtained at such parts of the Earth as are suitable from geographical considerations, while still lying sufficiently close to the points indicated by calculation as the best. It is easy to see how the shift of the planet on the Sun's disc, as seen from different stations on the Earth, can be determined by observing the duration of the transit. For the time occupied by the transit being known, the length of the chord followed by the planet in its passage is known. The length of the similar chord is determined from observations at the second station. The difference between the observed times of transit at the two stations depends, in fact, on the different lengths of the two chords. The calculator is then able to find the actual amount by which the apparent place of the planet has been shifted, and this, as we have seen, enables the distance of the planet to be discovered. The advantage of this process lies in the fact that, as the duration of the transit is the element to be observed, it can be determined by each observer in total independence of the observations of the other. If it had been found practicable to observe the duration of the transit with the precision which was at one time expected from this process, the Halleyan method for determining the scale of the Solar System by the Transit of Venus would probably be superior to all other methods of solving the same problem. For the difference which has to be dealt with, being not less than twenty minutes, is sufficiently large to give a very accurate result, provided the error with which it is affected be not more than a single second. But here is where the difficulty comes in. It was at first thought probable that the phenomenon designated as Internal Contact admitted of being observed with complete precision. But we now know that this is not the case. The movement of the planet is so slow that about eight minutes are required for Venus to enter completely on the Sun's disc, after first contact with the Sun's margin. In consequence of this tardy movement, the determination of the moment of Internal Contact admits of no very great precision. It has been found that different astronomers, working beside each other with different telescopes, have varied by as much as half a minute in their estimates of the time of Internal Contact. Nor do the difficulties arise solely from the slow movement of the planet; the limb of the Sun is not usually defined with sufficient sharpness to permit much exactitude in measurement. There is also a phenomenon due to irradiation which adds a special and quite indeterminate element of uncertainty to such observations. A dark object when viewed on a bright background is thought to be smaller than a bright object of equal size when viewed on a dark background. This is a well-known physiological fact, and serves as the explanation of not a few illusions. Never, however, has any such illusion been the subject of so much controversy as that which is called the "black drop" in the Transit of Venus. Just as the planet has entered entirely on the Sun's limb, and just when the actual Internal Contact is taking place, irradiation seems to make the black circle shrink. At the moment, however, the black spot is still at one point in contact with the bright limb, so that the appearance produced is that of a black band connecting the dark Venus with the brilliant edge of the Sun. So long as this black band exists the phase of Internal Contact cannot be said to have passed. In fact, it is the moment when this band disappears which the astronomer is so solicitous to obtain with precision. Much uncertainty has, however, arisen from phenomena connected with this black band. Some of the observers, perceiving the planet to be entirely on the Sun, though the black band remained, concluded that the critical moment had already passed. It has accordingly been necessary to make allowance in some of the observations for the presumed effect introduced by irradiation. It is obviously impossible to define any strictly logical grounds on which a correction on this account can be estimated. Observations which require interpretation and correction of this rather indefinite character seem unsuitable for the precise solution of such a question as the scale of the Solar System. In the observation of the transit by Halley's method, there are no fewer than four of these Internal Contacts, each of which has to be made the subject of a distinct time-determination. No insignificant degree of uncertainty is thus introduced into the period of twenty minutes or so on which the calculation depends. An indeterminateness to the extent of half a minute might very easily arise from the cause now under consideration. The result would be affected to the extent of one-fortieth of the whole, and the distance of the Sun concluded from such observations would be uncertain by not less

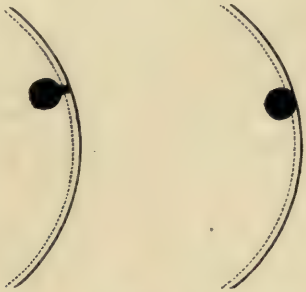


FIG. 12.—THE BLACK DROP IN THE TRANSIT OF VENUS.

than two or three per cent. of the total amount. An error to something like this extent was indeed made in the very earliest determination, which assigned to the Sun a distance of 95,000,000 miles. This was, as we now know, about two millions too much. A revision of the whole question by other methods, with a rediscussion of the significance to be attached to the meaning of the Internal Contact as noted by different observers, seemed, as was at first supposed, to point to a reduction of the distance to about 91,000,000 of miles. A final investigation has, however, shown conclusively that this result lies about as far on one side of the truth as that of the earlier investigation did on the other. It is now generally believed that 93,000,000 of miles is the true expression of the Sun's distance if only two significant figures are involved. If, however, we desire three significant figures, then 92,700,000 should be used. It is, however, quite possible that the seven, which is the last of the three, may be in error by one or even two digits. The ultimate object aimed at is to secure the accuracy of this third figure. Were it certainly known, then the Sun's distance is determined to within its one-thousandth part.

One great drawback to the employment of the Halleyan method arises from the fact that its success depends on having clear skies at the four observations of Internal Contact. If it should happen that at either of the two selected stations the observations are interfered with at the beginning or at the end of the transit by clouds, or indeed by any other cause, then the whole attempt becomes abortive. It is, therefore, very desirable to devise some other method demanding a less complete degree of success in making the observations. A process has accordingly been profitably employed, by which, from observations of a single contact made at two suitably chosen stations, the distance of Venus from the Earth can be discovered. This method is generally known as that of Delisle, to distinguish it from the Halleyan process, in which both ends of the chord of transit have to be observed. The selection of stations best suited for the Delisle method is guided by principles somewhat different from those which point out the two stations from which the Halleyan observations are made. The object now is to choose two spots on the Earth, so that from one of them the Internal Contact of Venus and the Sun shall be accelerated as much as possible, while from the other station the same Internal Contact shall be retarded as much as possible. If the Earth were but a point, so to speak, in comparison with the distance of Venus, then it is obvious that the moment of internal contact must be the same from whatever part of the Earth the phenomenon be witnessed. As, however, the diameter of our globe is appreciable in comparison with the distance at which Venus is situated, there must in general be some difference between the times at which the contact is observed from various stations. The exact determination of that time-difference in any particular case will enable us to compare the diameter of the Earth with the distance of the planet. Everything, therefore, turns on our possessing the means of learning with precision the actual interval which has elapsed between the observations made at the two stations.

At this point we are confronted with a difficulty which did not arise in the use of the Halleyan process. The observer at one station is now obliged to use some means of so recording the moment of contact that it shall admit of comparison with the similar observation made by the other astronomer. Each observer can, of course, determine with all needful precision the actual moment by his clock at which the phenomenon he observes takes place; but how is he to obtain a view of the clock which the other astronomer is using? Of course if a telegraph wire connected the two observatories, then the astronomer who occupied the site for accelerated ingress would be able to make a signal at the moment of Internal Contact to the other astronomer thousands of miles away, who would note the corresponding moment. Then, when he subsequently witnessed the Internal Contact, a glance at his clock would show him the precise number of minutes and seconds which had elapsed between the two observations. This would certainly be the most satisfactory method of proceeding. Perhaps, indeed, it is the only one which would afford a reliable result. Telegraphic communications, however, were not available with sufficient completeness during the last transit to render this method capable of the desired applications. We were, therefore, compelled to resort to other means for enabling the two distant astronomers to compare their time-determinations. Suppose a chronometer, which had been carefully rated by the clock at one observatory, be conveyed to the other, a comparison between the two clocks might thus be made, and the required difference of longitude obtained. This process, however, is not only very troublesome, but it hardly possesses the requisite precision. Even if a large number of chronometers be carried to and fro between the two stations, it is difficult to determine in this way with sufficient accuracy the difference between the longitudes of the two stations. For the successful application of this method for observing the Transit of Venus it is, however, essential that the longitudes of the two stations should be determined with a high degree of precision. Even if this difficulty were overcome, there still remains the phenomenon of irradiation, which of course affects the second method of utilising Transits of Venus, just as it does the earlier method already described. No doubt, in the transit of 1882, photographic processes were employed with considerable success, an arrangement being introduced whereby, just as the critical moment was approaching, a photograph was taken at each second of the Sun's limb, with Venus projected thereon. From among these photographs one was selected which appeared to be that on which Internal Contact was most exactly represented. There is, however, even with the help of the photographs, still some uncertainty about the matter; for observations of the moment of contact are gravely impaired by the slowness of the motion. It is like trying to tell the time on a clock accurately to a single second when you have only the minute hand to look at, or like trying to tell the minute if only the hour hand was available.

The Transit of Venus, however, has now but little more than an historical interest for astronomers. No doubt it has given us our first knowledge of the Sun's distance, but it has already been superseded by other processes. As more than a century must elapse before a transit will happen again, we may reasonably conclude that we shall have learned the Sun's distance, with much greater precision than any Transit of Venus could possibly give us, long ere our successors have the opportunity of witnessing the great phenomenon. The defects of the method are inherent and apparently irremediable, and in any case the fact that minor planet observations can be brought to bear upon the question in almost any volume that may be required, seems to make it certain that they must win in a competition with another method which depends only on single observations separated by long intervals of time.

CHAPTER IV.

THE SUN'S DISTANCE AND THE VELOCITY OF LIGHT.

WE have yet to explain a very beautiful line of reasoning by which we are enabled to obtain the value of the Sun's distance by researches into the velocity of light. The process is indeed one of the most recondite in the whole range of natural science, and it will merit our close attention. In this research we no longer employ the diameter of the Earth as the standard of comparison. We have already found many difficulties to arise from the circumstance that the Earth's diameter forms too short a base for convenient triangulation. This obstacle does not impede the use of the new method, though, as we shall presently see, difficulties characteristic of it are not wanting.

The inquiry into the Solar distance by the physical method is indeed a research of much interest from a scientific point of view. On the astronomical side it is connected with the remarkable discovery of the velocity of light made by Roemer, from his study of the phenomena attending the eclipses of Jupiter's satellites. It is also intimately associated with a classical event in the history of science—namely, the determination of the aberration of light, through the immortal researches of Bradley. The remarkable character of the phenomenon, and the exquisite process of reasoning by which Bradley arrived at its discovery, combine to lend interest to the employment of aberration for the purpose now under consideration. Then on the physical side we deal with an experimental problem which has taxed the skill and refinement of philosophical methods. The difficulties have, however, been overcome by a marvellous application of mechanical skill; and the velocity of light now seems to be known quite accurately enough for the determination of the Solar distance with the specified degree of precision, provided that the logical position of all the other elements of the problem be unimpeachable. Herein, however, lies the weak point of the physical method of finding the Sun's distance.

I do not know any branch of experimental research of a more delicate character than that by which the velocity of light has been determined. There is a subtlety in the method which can only be realised when the extraordinary nature of the problem to be solved is fully understood. For, think of the velocity of an express railway train as it dashes past a platform. It seems to approach, to pass, and to vanish in a moment. Then, stand near a rifle range, and note how rapidly the impact of the bullet on the target succeeds the flash accompanying its discharge. The rifle-bullet takes but little more than a second to traverse the same distance which the train accomplishes in a minute. Rise one step higher: look at a meteor as it dashes into our atmosphere, to perish in a streak of splendour. The meteor moves far faster than any rifle-bullet—in fact, its velocity is nearly one hundred times greater than that of the missile from any weapon which human hands have ever fashioned. Surely it would seem, at first sight, that the speed of a meteor must be as great as any speed which it would be possible for us to investigate? But we have not yet nearly reached the velocity which we have to deal with in the case of luminous undulations. We have to make a far greater advance. Think of the speed at which a little child runs across the room, and think of the speed with which a shooting star darts across the sky. Then work out the following sum in proportion. As the velocity of the shooting star exceeds the velocity with which a child can toddle, so does the velocity of light exceed the velocity of the shooting star.

Does it not seem almost impossible that we should be able to measure a speed so prodigious as that thus indicated? Light hurries along at a pace ten times as swift as that with which the electric message flashes along an ordinary telegraph wire. Sunbeams hasten towards us with their marvellous story at a speed one hundred times as great as that with which the cable conveys beneath the waters of the Atlantic the signals committed to it. The most rapidly moving star known in space does not move along with one-thousandth part of the speed imparted to the light which it radiates, and by which alone we become aware of its existence. Our own Earth voyages along its orbit at a speed of eighteen miles a second. Each breath that we draw is inhaled at a distance of a score or more of miles from where the last inspiration was taken. But what is the speed of our Earth when compared with the speed of light? The movement of our Earth is not animated with one ten-thousandth part of the velocity at which the beams are thrown from a little candle. Who does not know the terrific explosion which follows from the ignition of a mixture of oxygen and hydrogen; how instantaneously ignition is diffused throughout the volume of the exploding gases? But what is that velocity to the velocity of light? The speed with which the ignition was propagated should be magnified a hundred-thousand-fold before it could rival that with which the accompanying flash of light started on its journey. When Krakatoa was convulsed by the most frightful volcanic eruption of which there is any record, the tumult of that awful catastrophe was conveyed by aërial vibrations thousands of miles away. But notwithstanding the unparalleled impulse which the air received, it could only discharge its duty as a sound-carrier with a velocity the millionth part of that with which the ether transmits the undulations of light. In the mightiest typhoon by which towns have been overturned, by which ships have been driven ashore, and by which thousands of lives have been lost, the wind which wreaks this dire mischief has never attained the ten-millionth part of that velocity with which a beam of light pierces harmlessly through the delicate membrane of a dragon-fly's wing. Halley's comet, now on its way back to our system, has but recently left the remoter parts of its orbit. Comets sometimes move rapidly, but it may be well to note that during the long years when Halley's comet was nearly at its greatest distance from our system, it accomplished in a single second no more than the hundred-millionth part of the journey that light makes in the same time. The blood that hurries through our arteries would have to be quickened a thousand-million-fold before it would approximate to the speed with which the waves of light fall on the retina; while the eye of the reader, as he follows these lines, passes along the text with a velocity which may be regarded as the million-millionth part of the speed with which the message from the words is transmitted to his pupil.

Such being the velocity of light, need it be said that elaborate devices are required if it is to be measured accurately by any instrumental contrivance? I shall here briefly describe the principle of one of the most exact methods by which it has been determined.

Fig. 13.—Method of Measuring the Velocity of Light.

Stand in front of a looking-glass, and suppose that there is a screen moving between your face and the glass (Fig. 13), and that there are apertures A, B, C, in the screen, with intervening panels of equal size. Of course, when an aperture comes immediately before your face you are able to see your reflection in the glass, and then there is an eclipse until the next aperture comes in front, and so on. This seems obvious enough; but now suppose that while the screen remained where it was the mirror was removed to a long distance of many yards, or of a mile, or, better still, of ten miles. Of course you would not actually be able to see your reflection in a mirror, under any circumstances, ten miles distant. But that is not material; telescopes can be called to our aid, and a reflection can be seen sufficiently for our present purpose. We, therefore, simplify matters by imagining that an observer could see himself in a mirror ten miles away. When an aperture, A, in the screen comes directly in front of his face the light from his face has to travel ten miles to the mirror and ten miles back again. This occupies time, for though the light moves very rapidly its velocity is not infinite. Still, only the ten-thousandth part of a second or so will have elapsed, and if, in that time, the screen has not moved far enough for the panel between A and B to have cut off the view, he will duly see his reflection in the aperture A. If, however, the speed of the screen be sufficiently great, then the panel intermediate between A and B will be in front of his face by the time the light gets back again. It will consequently be intercepted, and he will see nothing. Let us suppose that the velocity of the screen is still further increased; it may then happen that during the journey of the light over the twenty miles the screen will have moved so far that the panel between the openings A and B will have moved out of the observer's way, so that he will be able to receive the beam on its return through the aperture B. Again, let the experiment be repeated with the speed of the screen once more augmented. The journey of the light for those twenty miles, after passing through A, will have carried the aperture B, as well as the panel between A and B, past the observer, so that the path will be blocked by the panel between B and C. If, however, the experiment be once more repeated with a still higher velocity of the screen, the aperture C may be brought into view; that is to say, by the time the light has moved from A to a distance of ten miles and back again, the screen has moved over the determinate interval from A to C. We thus get the velocity of light relatively to the velocity of the screen, and the latter being known, the velocity of light is ascertained.

It need hardly be said that the practical application of this principle involves a multitude of elaborate contrivances, into which I shall not here enter. The final result is that the velocity of light may be taken as about 186,300 miles per second.

There are two different methods by which the scale of the Solar System can be deduced, when the velocity of light has been ascertained by experiment. The first of these depends upon Jupiter's satellites, which in the course of their revolutions around the great primary occasionally plunge into the shadow of the mighty planet. The sunlight, which alone renders the satellite visible, being thus cut off, it ceases to be a luminous point, and to us who witness the phenomenon from afar, the phenomenon of an eclipse is presented. These eclipses recur so frequently that so far as the repetition and accumulation of observations can afford accuracy in such an inquiry, abundant information seems attainable. But for our present purpose the observation of an eclipse is defective, inasmuch as it is not a very sharply-defined phenomenon. The satellite is a globe 2,000 miles or more in diameter, and from the moment of its first contact with the shadow to the moment of its complete extinction an appreciable amount of time intervenes. What, then, are we to call the exact moment of the eclipse? Is it when the light first begins to decline? or is it when the light totally vanishes? The observed time of either depends on the dimensions of the telescope which the astronomer is using, and, indeed, in some measure on the atmospheric conditions as well. For these reasons it has been found impossible to prepare what the astronomer refers to as the Tables of Jupiter's Satellites with as much precision as would be necessary if we were to base so important a determination as that of the Solar distance on such observations. In order, however, to explain the character of the process by which Jupiter's satellites can be made to declare the Sun's distance, I will assume that the phenomenon admits of being observed with perfect precision, and that, by the discussion of observations of satellite-eclipses accumulated during past centuries, astronomers have obtained a knowledge of the movements of these little bodies so minutely accurate that they can predict their phenomena with precision. The Tables would then exactly set forth the time at which each eclipse took place, but this would not be the time at which it was witnessed here. Dwellers on this Earth are at a distance of hundreds of millions of miles from Jupiter, and consequently the intimation of the occurrence of any phenomenon in Jupiter's system has to travel across hundreds of millions of miles before it reaches our eyes. Suppose, however, that we noted the moment at which an eclipse of Jupiter's moon was observed here, and suppose we knew from the Tables the precise moment at which, as a matter of fact, the eclipse did occur, then the interval of time in minutes and seconds between the two epochs would represent the time required by light to travel across that span of the Solar System which intervenes between Jupiter and the Earth. If we knew this time with accuracy, and if we also knew the velocity with which light travels, we should then have the means of finding the length of the journey. Now, the velocity with which light travels is an element which has been made the subject of careful experimental determination. As we have already said, it may be regarded as known with sufficient precision. But there is still a question as to whether the velocity of light in air—which is, of course, the only medium in which we can measure it—may be regarded as equivalent to the velocity of light in its movement through open space. It may, indeed, be admitted that it does not seem likely, either from experimental investigations or from theoretical considerations, that there should be any material difference between the velocity of light in air and its velocity in space. For light consists of undulations of the ether, and the fact of ether being sparsely occupied with molecules of gas does not seem to necessitate any change in the velocity with which the luminous undulations are transmitted. There is, therefore, good reason to hope that, so far as the velocity of light is concerned in the investigation of the Sun's distance, no substantial uncertainty need be apprehended. The ascertained value of that velocity is believed to be accurate to within less than one-thousandth part of its total amount. It may also be remarked that this is one of those constants on which the labours of successive experimenters may be accumulated, so that the result ultimately obtained acquires a precision that could be hardly expected were the earlier endeavours alone available. The question then arises as to how far the time occupied in the journey of the light from Jupiter to the Earth can be determined with accuracy. The interval is at most sixteen minutes and a quarter, and in order to come up to the required standard of precision in the determination of the Sun's distance, this period must be ascertained with an uncertainty of less than one-thousandth part of its total amount. It is, therefore, easy to show that the period must be known within a second of its true value, and it should be added that the assumed time limit of sixteen and a quarter minutes is so very extreme that it is often impracticable to attain a range so great. It must, then, be admitted that there is no likelihood of our discovering this period with anything like the accuracy which would be necessary if we were to base on it a determination of the Solar distance which should be trustworthy within the specified limits. We have to contend against the uncertainty of the observation of a phenomenon so indefinite as the moment of the eclipse of the satellite; and we have also to reckon with the imperfections, already referred to, of the Tables of the movements of Jupiter's moons. It seems hardly likely that any satisfactory method for determining the Sun's distance can be expected from such observations.

But there is another method by which we can employ an experimental determination of the velocity of light to evaluate the distance of the Sun. This method is one of great elegance, depending as it does upon that exquisite phenomenon known as the Aberration of Light. What this is we must now proceed to explain. Suppose a rifle bullet were fired at a railway carriage in a direction perpendicular to the sides of the carriage. It is obvious that if the carriage were standing at rest on the line, the bullet would leave the carriage by a hole which was at the same distance from the front end of the carriage as was the hole which it made at entrance. If, however, the carriage were moving along the line, it is equally obvious that the advance of the vehicle during the passage of the bullet through the carriage would make the bullet emerge at a point somewhat further from the front end of the carriage than was the hole at the other side; while, if the carriage had been running backwards, the hole at leaving must be nearer the front end than the hole at ingress. I have endeavoured to explain what is meant in the diagram,

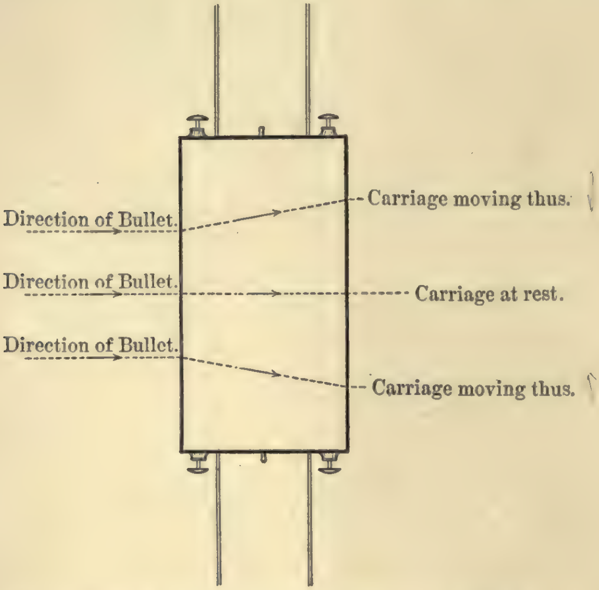


FIG. 14.—TO ILLUSTRATE THE PHENOMENA OF ABERRATION.

Fig. 14. It is of course to be noted that the movements have been necessarily much exaggerated.

Now, though the bullet was really fired in a direction perpendicular to the rails, yet an occupant of the carriage who judged as to the direction whence the bullet had come from the position of the holes which it made, would form an altogether erroneous conclusion if he overlooked the effect which the motion of the carriage had on the apparent direction of the bullet's flight. The fact is that the motion of the carriage produces in it an apparent alteration. This is a phenomenon precisely resembling that known as the Aberration of Light.

Bradley had discovered that each star is apparently displaced from its true position in the heavens, in consequence of the fact that light moves with a finite velocity. The spot in which a star is actually situated is not precisely the spot to which the telescope of the terrestrial astronomer must be pointed when he wishes to observe that star. Let it be carefully understood that the phenomenon to which we are now referring is of a wholly different character from the phenomenon of refraction by which the apparent place of a star is removed from its true place. The latter is merely a result due to our residence at the bottom of an ocean of air through which we look at the stars, the line of vision being broken in passing out of this medium into space, just as the line along which a fish might look out from his element at a bird flying in the air, would change its direction as it traversed the surface of the water. But the phenomenon of aberration would be just the same if there were no atmosphere whatever. It depends on the fact that the velocity with which light moves, great though it be, is still not immeasurably greater than the speed with which the Earth moves in its orbit. Speaking generally, we may say that the average pace of the Earth is about eighteen miles a second, and the speed with which a ray of light moves is about ten thousand times as great. It can be shown, that to observe a star with a telescope, situated as we are on a globe hurrying along at eighteen miles a second, we must point the instrument not exactly at the star, but a little in advance of it, in the direction in which the Earth is moving.

And now for the method by which we are able to elicit the distance between the Sun and the Earth from the phenomenon of the Aberration of Light. It is possible by protracted observation of the stars to determine the amount by which their places are affected by aberration. This does not, it must be observed, involve any assumption as to the movements of the Earth, for we now refer merely to what is actually found by telescopic research, as to the places of the stars. To take a simple case, let us suppose that a star is actually situated at the pole of the ecliptic; indeed, any star which happens to lie close to that point will sufficiently answer our purpose. If we employ the resources of our modern instruments to determine the place of this star at any particular date, it will not appear to us to lie at the very point that we know it occupies; the apparent place of the star will be at a distance from the true place, the angular magnitude of that distance being about 20·4 seconds. Suppose that the observation be repeated in a week or a month, and at various other intervals; in the course of a twelvemonth we should find on every occasion that the star always lay 20·4 seconds distant from its true position. The actual place which it occupies does not, however, remain the same; it moves round in a little circle, so that in the course of a year it has completed a circle with a radius of 20·4 seconds round its central position. It will thus appear as if the star were actually in motion. It is quite true that in the majority of stars the effects of the phenomenon we are now considering are not quite so simple as in the case of one lying near the pole of the ecliptic. Generally speaking, the apparent orbit which the star describes in virtue of aberration is an ellipse, gradually becoming more and more eccentric the further it lies from the pole of the ecliptic, until at the ecliptic itself the ellipse has become contracted into a straight line, about 40·8 seconds long. From one end of this line to the other the star moves in the course of six months, and in the course of the ensuing six months retraces its steps to its original position. The discovery of the Sun's distance by this method depends on the determination, with the last degree of accuracy, of the diameter of the circle or ellipse, which the star describes. Many astronomers have lavished on this problem the utmost skill and care in the selection and treatment of their observations. Among those who have laboured at it with especial success, we may mention the illustrious astronomer Struve, who investigated the Constant of Aberration in the case of seven stars which pass near the zenith of Pulkowa, the site of the famous Russian observatory. The several stars gave a wonderfully concordant result, and the value of the Constant of Aberration elicited from the entire series is 20·445 seconds. It is not, indeed, contended that this result is actually reliable to the last thousandth part of a second, as the recorded number of digits would imply. Every determination of this kind has a "probable error," as astronomers call it, and the "probable error" in the present case is the one-hundredth part of a second. I ought, perhaps, here to explain that what an astronomer means by a "probable error" is a magnitude so related to the numerical determination arrived at, that it is equally likely the error which has, in point of fact, been committed, shall lie below, as that it shall lie above the "probable error." It seems not improbable that the co-efficient of aberration may justly be regarded even now as known with an accuracy approaching the one-thousandth part of its total amount. We may, at all events, hope in the course of time to learn the value of this constant with all needful precision. Assuming this, we can now show how to obtain the Sun's distance. It can be proved that the ratio which the number 206,265 bears to the Constant of Aberration is precisely the same as that borne by the velocity of light bears to the velocity with which the Earth moves in its orbit. We have seen that the velocity of light is known, and that the Constant of Aberration is known, and we may reasonably hope, as already pointed out, that both these magnitudes can be learned with a margin of error not exceeding one-thousandth part of the whole. We can thus determine with a corresponding degree of precision the velocity with which the Earth moves in its path. It is true, of course, that the velocity of the Earth is not constant, for since its orbit is elliptic, the pace at which it is traversed is now greater, and now less, in accordance with the precepts of Kepler's law. But these are difficulties which the mathematician obviates without much trouble; they need not be further referred to, and we may treat the orbit as if it were actually circular. The velocity of the Earth having been thus determined, we can compute how far it moves in an hour, or in a day, or in a year. That is, we know the circumference, whence we can deduce the diameter of the Earth's orbit, and so reach the object of our investigation—namely the mean distance of the Sun from the Earth.

It must be admitted that however great may be the intrinsic beauty of this process, it still lacks certain elements indispensable to a really unexceptionable method of solving the great problem. It is not quite certain that the velocity of light and the velocity of the Earth are alone concerned in it; the very fact that the Sun is moving among the stars and bearing the Earth with it involves a certain complication, the extent of which remains at present undetermined. The physical considerations involved are of so profound a nature that we can never feel quite confident that some principle or condition almost wholly unknown to us may not have intruded into our result. We must therefore treat this process as inferior to those in which, by simple operations of surveying, we determine the distance of the Sun by methods of rigid accuracy.

Nothing is more remarkable to the student of modern astronomy than the way in which the Sun's distance appears as an element in astronomical theories with which, at the first glance, it might seem to have no connection whatever. Whenever it has been demonstrated that the distance of the Sun does contribute in some degree to the numerical value of an observable phenomenon, it is then possible, when other things are sufficiently known, to deduce the value of that distance from measurements of the phenomenon, notwithstanding the apparent slenderness of the connection. I will here give a remarkable illustration of this statement, and show how the distance of the Sun may probably ere long be determined from observations which at first sight appear entirely unrelated to it. Important aid has been rendered to astronomers by the spectroscope in the detection of the movements of heavenly bodies along the line of sight. It is one of the most singular facts connected with this process that the spectroscopic investigation indicates this component of velocity independently of other particulars. It does not tell us how large the body is, or how far off, but it does tell us whether it is approaching or receding from the Earth, as well as the velocity with which this movement is performed. As Sir W. Huggins, the virtual inventor of the method, has so well declared, it is not improbable that the information which the spectroscope will give as to the movements of the heavenly bodies may ultimately prove to be even more important than the information as to their constitution, which this instrument affords our only means of obtaining. Of late years the application of this method has been so greatly improved that the measurement of velocities along the line of sight can be accomplished with an accuracy which, until quite recently, would have been thought impossible. Indeed the rate of advance in this direction has been so great as to raise the hope that the best results have not even yet been reached, and that ere long we may have the means of determining the speed of stars along the line of sight with an accuracy even closer than that limit of one mile a second, already attained in the researches of Keeler and Vogel. When a measurement of the motion of a star along the line of sight has been obtained, we are not entitled to attribute the entire result indicated by the spectroscope to the star alone; we must remember that we are on the Earth and partake of its velocity. If it should happen that the star lay in the ecliptic in the direction of a tangent to the Earth's orbit, then the Earth would be hurrying directly towards or from that star at the pace of eighteen miles a second, and consequently the observed velocity along the line of vision would have, as the case might be, to be reduced or augmented by eighteen miles a second in order to obtain the true pace of the heavenly body. If, on the other hand, the star happened to be situated at the pole of the ecliptic, then the velocity of the Earth in its orbit, being at right angles to the line of sight, would contribute nothing to the apparent motion of the star along the line of sight, and consequently the velocity indicated by the spectroscope might be legitimately attributed to the motion of the star alone. In general, of course, we should, before concluding as to the actual motion of the star, have to correct its apparent motion as indicated by the spectroscope for the component of the Earth's motion along the same direction.

Let us now suppose an immense multitude of such observations, referring to many different stars, to have been made. Let us further suppose that we knew the Earth revolved in an orbit around the Sun, which for our present purpose may be regarded as a circle, but were ignorant of the rate of its motion. It would then be possible from our spectroscopic observations of the stars to determine the velocity with which the Earth's movement was conducted. This may seem a very recondite method of investigation, and I do not say that at the present time sufficient materials for applying it are obtainable. It is, however, very interesting to note that these lines of sight determinations afford in principle the means of finding the Sun's distance. For say the Earth moves ten miles a second; then let us apply a correction on this assumed value to all the observed motions of the stars along the line of sight. A certain series of rectified movements will result. Let us now take some other pace for the Earth, say twelve miles a second; acting on this assumption we can compute the corrections to be applied to the observed radial motions of the stars, with the result of obtaining a second series of inherent motions. Similarly we can make several other suppositions for the Earth's velocity per second, such as fourteen, sixteen, eighteen, twenty, twenty-two miles, etc. In each of these cases, by applying the appropriate corrections, we can deduce a system of numbers purporting to be the actual velocities of the stars along the line of sight, on the corresponding assumption as to the velocity of the Earth in its orbit.

Among all these different systems of possible motions, one is true and the rest are false—which is the true one? Here, as elsewhere, with the theory of probabilities for our guide to the discovery of truth, we can form a mathematical expression for the probability of each of these different systems. One of them must be true, and if a sufficient number of different observations have been embodied in it, we can lay our finger on the true one, owing to the simple fact that it is the system which has the highest probability in its favour. Thus we learn the orbital velocity of the Earth; and since we know that the Earth takes 365¼ days to perform one circuit, we can determine the length of that circuit. Having reached this point, a simple calculation gives the diameter of the Earth's orbit, and half that diameter is the distance of the Sun. Here then is a conceivable method by which, through spectroscopic observations of the movements of stars along the line of sight, it becomes possible to discover the value of that fundamental constant of astronomy —the distance of the Earth from the Sun.

CHAPTER V.

THE MASS OF THE SUN.

I THINK it was Professor Sylvester who declared that he never felt sure he thoroughly understood any mathematical theory until it had become so familiar to him that it almost seemed as if he could explain it to the first man in the street whom he met and button-holed for a few minutes' conversation. I have to enter in this chapter into some account of matters which are usually considered to lie wholly within the domain of mathematics. They are generally expounded with the aid of symbols, and by means of calculations, which would be quite out of place in the pages of a volume like the present. Should any reader of this work desire to pursue this branch of our subject, with the illumination which mathematics alone can afford, there are plenty of books forthcoming to give him the necessary assistance. I may here specially refer to Professor Young's "General Astronomy." In this work he will find the matters which are now to occupy us discussed in an admirable manner, without the introduction of analytical subtleties, which might perplex the student who has only a limited acquaintance with mathematical processes. Referring those who possess the requisite training to a work like that I have mentioned, I shall here endeavour to give a description of the investigation adapted to the general reader. I shall follow, in so far as it may be in my power, what I presume must have been the method so graphically indicated by Professor Sylvester when striving to convey some information about his mathematical researches to some non-mathematical acquaintance.

To those astronomers who are most competent to judge, the gravitational method of finding the Sun's distance appears of the utmost importance. It is intimately associated with the most brilliant mathematical efforts by which science has been enriched. The names of Newton and Lagrange, to mention two very great names only, are specially connected with the superb discoveries on which this method of determining the Sun's distance depends. Indeed, this mode of investigation appeals so forcibly to mathematicians that we can hardly feel surprised if the enthusiasm with which they regard it has sometimes led them to adopt, perhaps, a somewhat disproportioned view of its merits, as compared with those humbler methods of investigating the same problem which make a less fascinating appeal to our intellectual powers. As an illustration of what has here been said, I may mention that the illustrious French astronomer, Le Verrier, who shares with Adams the immortal glory of having discovered the planet Neptune, has marked, in a very emphatic manner, his sense of the superiority of the gravitational method of determining the Sun's distance over all the other processes. At the time when most other astronomers were busily employed in making due arrangements for the observation of the pair of Transits of Venus which occurred in 1874 and 1882, Le Verrier almost alone stood aloof. He thought that any information as to the distance of the Sun which these transits could yield, was not worth the trouble which it was proposed to undertake. He felt confident that though the gravitational method could not at that moment be said to have solved the problem with the desirable accuracy, yet that it was destined ultimately to do so. For when, after the lapse of a century or two, sufficient observations of the planets had been accumulated, the gravitational process would become far more reliable as a method of finding the Sun's distance than the Transits of Venus ever could be. Under these circumstances it appeared to Le Verrier that the great trouble taken in observing the Transits of Venus, and the large expense incurred, were unlikely to be justified by the result. The majority of astronomers, however, did not take this view, and consequently the Transits of Venus were studied with a degree of care and attention never before bestowed on any similar phenomena. As there has been so much difference of opinion on the subject, I may perhaps be permitted to mention my own view of the matter. It will be evident, from the chapter in which I have discussed the Transits of Venus, that there are very serious objections to this process of determining the Sun's distance. Indeed, so serious are they, that I entertain no doubt as to the correctness of Le Verrier's contention as to their inferiority to the gravitational process. At the same time, it seems impossible that the gravitational, or any other indirect method, can ever come ultimately to be regarded as the most reliable means of fixing the scale of the Solar System. It seems necessary to emphasise the fundamental truth, that the determination of that scale is a problem of mere geometrical surveying, into which theoretical considerations must be introduced as sparingly as possible. The Transit of Venus method, no doubt, is of a geometrical nature, but it is open to characteristic objections, which largely reduce its value. The minor planet method is also a geometrical process, involving no recondite theoretical considerations. It is free from the objections which affect the Transit of Venus, and it appears to me to be demonstrably the most perfect of the geometrical processes. I am, however, free to admit that my mathematical sympathies are entirely with Le Verrier; for the gravitational process inspires me with a degree of interest which I can feel in none of the other methods. This is chiefly due to the intimate association of the process with many other astronomical questions of the utmost importance, even though, at first, it may seem to have no connection with them.

To begin at the beginning, it would be first necessary to ascertain the weight of the great luminary. I do not mean to determine the number of tons that it contains; such is not the standard of weight now to be employed. We must adopt an unit more consonant to the scale of the problem on which we are engaged. The unit of mass adopted must be the mass of our own Earth, and what we have to investigate is the number of times that the Sun outweighs the Earth. In other words, supposing a gigantic balance were constructed, and that the Sun were placed in one pan of that balance, how many globes, each as massive as the Earth, would it be necessary to place in the opposite pan, so as just to produce equilibrium? It is easy to see that the mass of the Earth is a suitable unit for the expression of the masses of other celestial bodies, just as the distance from the Earth to the Sun is the most convenient unit in which to set forth celestial distances generally.

We might certainly anticipate that the Sun would be found greatly to exceed the Earth in mass. It is of more than one hundred times the terrestrial diameter, so that even though it be largely composed of gaseous material, the mass of the orb of day must still obviously be many thousands of times that of our globe. We want, however, to obtain exact knowledge on this subject. It would be necessary to do so in order to arrive at an adequate conception of the chief physical characteristics of the Sun. And such knowledge is essentially requisite for the determination of the Sun's distance by the gravitational method. It may, indeed, seem strange that, from knowing how heavy the Sun is we should be able to learn at what distance it is situated from us. But such is the fact.

Our methods of weighing the Sun comparatively to the Earth depend upon assistance rendered by the neighbouring planets, Venus and Mars. The matter is by no means a simple one, but I think it will be possible to make it understood, without the introduction of mathematical symbols. Let us first consider the orbit of the planet Mars, which revolves, as we know, in an elliptic track outside the orbit of the Earth. If Mars were a solitary planet revolving around the Sun, then the orbit in which it moves would remain unaltered between one revolution and the next; indeed, the planet would trace out, century after century, exactly the same track in the heavens. The presence of the Earth, however, not to mention any of the other planets, introduces a disturbing element. Just as the Sun attracts Mars, and by that attraction retains the ruddy globe in its elliptic path, so in like manner the Earth attracts Mars, and compels it to swerve to some extent from the path which it would otherwise pursue. I shall have occasion to refer more fully to this matter of the mutual attraction between the Earth and the planet in a subsequent chapter on the subject of climate. We shall there be occupied with the consideration of the disturbances which the Earth experiences in consequence of the attraction of the planets. We are now, however, to direct our attention to the disturbance which the planets experience in consequence of the attraction of the Earth. It is quite obvious that if our globe were an excessively minute body in comparison with the Sun, then the effect upon Mars caused by the disturbing action of the Earth would be inappreciable. On the other hand, if the Earth were a globe as massive as the Sun, the motion of the planet would be reduced to utter disorder. Under such circumstances it could not be said to revolve around the Sun more than around the Earth; its course would not then be an ellipse, or anything like an ellipse. It is, therefore, plain, from the fact that Mars' movement is so nearly an ellipse with the Sun in the focus, that the mass of the Sun must greatly exceed the mass of the Earth. As, however, some perturbations distinctly perceptible in the planet's movements are attributable to the disturbance produced by the attraction of the Earth, it is evident that, small though the Earth may be in comparison with the Sun, it is yet not wholly insignificant. A little further consideration will show that, from the elements now before us, it ought to be possible to determine the ratio of the masses of the Earth and the Sun, by studying the effects which they produce on Mars.

The perturbation affects the orbit of the planet in several distinct ways; for the sake of clearness, however, I shall refer only to one particular element of its movement. Draw an ellipse to represent the orbit of Mars. If that orbit were undisturbed by the attraction of the Earth, all other planets being overlooked for the present, then the size of the ellipse, as well as the position which it occupies, must remain absolutely unaltered from age to age. Think then of that point of the orbit called the apse, which lies nearest the focus. This point would remain perfectly fixed if the orbit were undisturbed. Owing, however, to the perturbing effect produced by the Earth, the orbit of Mars experiences among other changes a certain shift round in its plane. The consequence is, that the apse is gradually moving. This is competent to produce after a sufficiently long time a very marked change in the phenomena exhibited by Mars. It is well known that the brightness of this planet varies considerably between one opposition and another. Its orbit has, indeed, an eccentricity unusually great for an important planet, and for this reason the distance of Mars from the Sun changes proportionately much more than the distances of Venus, Jupiter, or Saturn. The effect is immediately apparent in the variations in the brilliance of Mars at different oppositions. If, for instance, the planet happens to come into opposition at the same time with its passage through perihelion —as was the case, for instance, in 1892 and in 1877—then the brightness of the ruddy globe is especially conspicuous. If the orbit were never disturbed by the attractions of other planets, the apse would always lie towards the same part of the sky, and consequently Mars, when seen at its brightest, would always appear in the same part of the heavens. We have, however, seen that one of the consequences of perturbation is a motion of the apse, owing to which this point of the orbit moves in the course of time round and round in the sky; so that the part of the heavens where Mars is seen when its glories are displayed to greatest advantage is perpetually, though slowly, changing. A distinct influence on the phenomena presented by the planet can thus be attributed to the perturbations produced by the attraction of the Earth. The greater the mass of the Earth the more rapid accordingly will be the shift of the apse of the planet. Now, it is possible by a certain calculation, into the details of which I cannot here enter, to determine what the motion of the apse would be for any particular ratio between the masses of the Earth and the Sun. To illustrate the process followed by the mathematical astronomer in the solution of such a question, I shall give a description which will, at all events, suffice to explain the principle on which the work is conducted. We may begin by assuming what is otherwise certainly known—namely, that the Earth has a mass which is only a very small fraction of the mass of the Sun. As a first hypothesis let us imagine the Sun's mass to be 100,000 times as great as the mass of the Earth. On this supposition we can make a calculation as to the effect which the disturbance of the Earth would produce on the movement of the apse in the orbit of Mars. The investigation shows that the perturbation thus arising would be considerably larger than the perturbation actually disclosed by observation. Hence we conclude that the mass of the Earth cannot be so much as 100,000th part of the mass of the Sun. We therefore commence our computations anew, on the assumption that the mass of the Earth is much less than the amount previously adopted. We shall, for instance, suppose it to be the 500,000th part of the Solar mass—that is to say, we shall see what the effect would be if the mass of the Earth had a value only one-fifth of that previously adopted. Repeating the calculations, we determine the amount of movement in the apse which this particular value of the Earth's mass would produce, and the result turns out to be considerably less than that which observations of the planet have shown to be the correct value. It is thus obvious that our second assumption regarding the mass of the Earth was pitched too low, and that our globe must be heavier.

We have therefore learned from the study of the movements of Mars that while the mass of the Sun must be more than 100,000 times, it nevertheless must be less than 500,000 times, the mass of the Earth. We have, in fact, obtained two limits between which the Sun's mass must lie. By subsequent repetitions of the calculation we are enabled to draw these two limits closer together, and thus to approximate more and more closely to the actual value which the mass of our globe possesses. The degree of precision which can be attained depends of course upon the accuracy with which astronomical observations have determined the motion of the apse. The more precisely it is known, the more closely can the mass of the Earth be determined by the method indicated. One of its characteristics is, that as decades and centuries roll by, and as the movements of the ruddy planet are more and more studied with instruments of precision, it will be possible to find with more and more exactitude the number of seconds by which the apse of the planetary orbit advances during the course of a century. Once this is known with the accuracy that may with good reason be anticipated, we shall then have the means of discovering with satisfactory precision the mass of the Sun, or, rather, the ratio which that mass bears to the mass of the Earth. Up to the present it can hardly be said that our observations of Mars have lasted long enough to give the result with sufficient definiteness. Accurate observation of the places of the celestial bodies is a comparatively modern art, and, generally speaking, such measurements as have come down to us after the lapse of a couple of centuries are too rough to indicate with all the accuracy that is desirable for the present purpose the perturbations which Mars has experienced from the Earth's attraction. One circumstance of a somewhat accidental character has provided us with a position of Mars at a tolerably remote date, which is of very great value in this connection. It appears that on the 1st October, 1672, the planet happened to occult a star in Aquarius; by this is meant that on the occasion referred to, the movement of the planet caused its interposition directly between the eye of the astronomer and the star. The consequence was that the star disappeared for the short space occupied by the planet in passing over it. This observation was duly recorded by three independent observers, and the time of the disappearance of the star was noted with sufficient accuracy. And since the place of the star admits of being determined with great precision, the observation affords accurate information as to the precise locality in the sky occupied by Mars on the occasion in question. We have thus at our disposal one sufficiently ancient position of the planet, and this enables us to obtain a value of the movement of the apse. For the position which the planet occupies at any date depends on the position of this point. We can attribute to it a certain velocity, and we can calculate on that assumption where the planet would have been found on the date of the occultation. If the position thus found does not coincide with that occupied by the particular star over which Mars passed, then our assumed value for the movements of the apse are wrong, and we must repeat the calculation with another value. This time, perhaps, the adopted value would seem to shift the place of the planet to the other side of the star. This being so, we should learn that the true value for the velocity of the apse must lie between those two assumed provisional values. These attempts to determine its amount must be repeated, until at last a value has been arrived at which locates the planet precisely on that star in Aquarius, at the actual day and hour when it was observed to be there. Observations have in this way helped to determine the motion of the apse of Mars due to the attraction of the Earth.

From the explanation thus given it will, I hope, be understood how the observation of the planet Mars enables us to weigh the Earth against the Sun. I may mention that the mass of the Earth is about the 324,000th part of the mass of the Sun. We do not as yet pretend, for the reasons already pointed out, to have ascertained this magnitude with all the accuracy which we hope at some time to attain. It is, however, desirable to state the degree of precision that would be needed for our purpose. We have already mentioned that the ultimate object to be aimed at is the evaluation of the Sun's distance accurate to within 1,000th part of the total amount. One of the striking advantages of the present method of determining this important constant is, that we need not demand an equal degree of accuracy in the measurement of the Sun's mass. Strange to say, the calculation seems to improve on our original data. It will presently appear that the cube of the required mean Solar distance is proportionate to the mass of the Sun. Any small error in the determination of the mass will therefore, by the principle which will be presently explained, produce only one-third the same error in the deduced Solar distance. Accordingly, we need only demand one-third of the accuracy in the determination of the Sun's mass that we require in the evaluation of the Solar distance thence derived. In other words, we should be content if the observations of Mars could tell us the mass of the Sun correctly to within about a 300th part of its total amount. This approach to truth is quite within the limits of what may certainly be hoped for. Hence many astronomers have been led to regard this method with greater favour than can be bestowed on any other indirect method of resolving the great problem.

We have now to consider the second branch of our present subject, and that is the means by which the necessary connection is established between the distance of the Sun and the mass of the Earth, so that when one of these constants is known, the other is determined. Here, again, we have to deal with a somewhat complicated matter. Strictly speaking, its investigation would require a free use of the formulæ of mathematics. We shall, however, endeavour to give a general account of the matter, using for the purpose arguments of a simpler and more generally intelligible construction than those which the mathematician is accustomed to employ.

We are in the habit of speaking of the Earth as revolving around the Sun, or of the Moon as revolving around the Earth; and no doubt there is a certain convenience in regarding the larger of the two bodies as being at rest, and the entire movement as being performed by the smaller body. It is, however, necessary to observe that, though undoubtedly convenient, this mode of expression does not accurately describe what actually happens. It is not the case that one of the bodies is at rest, and that the other does all the running. No doubt this might be true if the mass of the smaller object were so minute that it bore a wholly imperceptible ratio to the larger one. Were a planet no larger than the mote in the sunbeam, it might perhaps with truth, or with a sufficient approximation to truth, be regarded as revolving around a Sun at rest, if that Sun were as large as our own, and if there were no other interfering planets. Strictly speaking, however, even in this case the greater body could not, in the mathematical sense, be held to be absolutely devoid of all motion. It must respond, though doubtless to an infinitesimal extent, to the attraction of the planet, even if that have no greater mass than is possessed by a mote in the sunbeam. The mass of the planet should be absolutely zero were it to be completely devoid of influence on the great object by which it is guided. Dealing, therefore, with the Solar System as we actually find it, it is quite obvious that though the Earth moves round the Sun, it is not correct to say that the Sun remains at rest.

As the reasoning on which we are about to enter is one that presents some little difficulty, I will endeavour to simplify it by the removal of every consideration not absolutely essential to the matter in hand. I will therefore assume that the orbits of the heavenly bodies are perfect circles; for, although this is not actually the case, yet the fact that the planetary orbits are ellipses, and not real circles, does not introduce any source of uncertainty, or affect our present line of reasoning.

To illustrate the argument, I will suppose an ideal system. Let us take a Sun as massive as our own, and a planet of imperceptible mass revolving in a circular orbit around it, at that same mean distance which it is the object of our investigations to determine. The period of revolution will be one year, and three elements are involved in the determination of that period. There is first the mass of the Sun, there is secondly the radius of the orbit described, and there is thirdly, the gravitational constant; that is to say, the actual intensity of the force with which two units of mass attract each other, when separated by the unit of distance. This constant is of such importance that it may be well to dwell for a brief space on its character. In "The Story of the Heavens" I have given an illustration of the magnitude of this force. I may repeat it here, in so far at least as may be necessary to give a notion of the actual quantities with which we are concerned. Let us suppose a globe of solid iron fifty yards in diameter; let us further suppose it placed at a distance of one mile from another globe of equal dimensions, and of the same material. Then the attraction between these two masses produced at that distance by gravitation would be equal to the force due to the pressure of one pound. The value of the gravitational constant depends upon that particular property of matter which is the cause of the force of gravitation. If the attraction between these two globes had been different from what it is, then the time that the Earth takes to revolve around the Sun would undergo a corresponding alteration. We thus understand that there is a distinct relation between the value of the gravitational constant and the planetary movements. Once this constant is determined, then the time of revolution of any planet would be known when the mass of the Sun was known, and the mean distance of the planet had been assigned.

The mass of the Sun having been determined relatively to the mass of the Earth, we shall now proceed to show how by that means the distance between the Earth and the Sun can be determined. I must, however, here first of all explain one of the artifices which astronomers are in the habit of employing to simplify the consideration of questions such as the present. It would obviously be of great advantage if we could regard one of the two bodies as fixed, and consider the entire movement as performed by the other while revolving around it. As already explained, this is not the case in Nature, for each of the bodies does, in fact, revolve around the common centre of gravity of the two. It can, however, be shown that the relative movement with which the planet performs its orbit around the Sun is precisely the same as it would be if the Sun were absolutely fixed, but had its mass augmented by adding to it that of the planet. In considering the motion of the Earth around the Sun, we can thus regard the Sun as at rest, provided we add to it the mass of the Earth. This last, however, is so small, being less than the 300,000th part of the Sun's mass, that any error arising from its omission may be regarded as quite insignificant.

It will, perhaps, make our argument clearer if we first of all pursue a course which will only give an approximate result; we shall subsequently point out why it is only approximate, and then be in a position to give the more exact method. For this step in our argument we must consider the movement of the Moon around the Earth. Our satellite revolves at a known distance of 240,000 miles, and the period in which it accomplishes its revolution is 27·3 days. This distance and this period both depend upon the mass of the Earth, augmented by that of the Moon, as well as upon that gravitational constant to which we have already referred. As, however, the mass of the Moon is only about the eightieth part of the mass of the Earth, we shall for the present neglect it, and consider how far the movement of the Moon is affected by the mass of the Earth alone. If we suppose the distance of our satellite unaltered, but the mass of the Earth increased, it is obvious that the attraction of our globe upon the Moon would also be augmented. If, therefore, the Moon were to remain in its present orbit, supposed, for convenience, to be circular, its speed must evidently be increased. For the attraction of the Earth is neutralised by the centrifugal force of the Moon; if, therefore, the attraction be increased, the centrifugal force must be likewise increased, if the orbit is to remain unaltered. This means that the velocity with which the Moon moves in its orbit must become greater, which involves a shortening of the periodic time—in other words, we learn that if the mass of the Earth were increased, the periodic time of the Moon should be diminished, in order to keep it revolving in the same orbit which it at present pursues. On the other hand, if the mass of the Earth were lessened, then the attraction exerted on the Moon would be diminished; so that if the Moon then retained its present velocity, its centrifugal force would be in excess of the attraction, and therefore the orbit would become enlarged. The only way in which the Moon's actual orbit could be preserved would be by diminishing its speed, which is, of course, equivalent to increasing its periodic time. We are now able to state the law which connects the mass of the Earth with the periodic time of the Moon. An astronomer would express it by saying that the square of the periodic time varies inversely as the mass of the Earth. In illustration of this, we may remark that if the mass of the Earth were increased fourfold, then the periodic time of the Moon, if its distance remained unaltered, would be just half what it is. If the mass of the Earth were increased to nine times what it is at present, then the Moon should revolve, if at the same distance, in one-third of its present time—that is to say, in a little over nine days. Or to take a very extreme case, let us suppose that the Earth were so enormous as to be equal in mass to the Sun. We should then be obliged to allow that its materials were of extremely great density, or, at all events, that they were much more dense than those of the Sun; indeed, it is well known, that if our luminary were placed with its centre at the centre of the Earth, its outer surface would far more than enclose the orbit of the Moon. Suppose, however, that we had a globe of platinum as heavy as the Sun, and occupying the same position as the Earth, we can show by the rule to which I have referred, that the Moon would have to revolve so rapidly that it should accomplish its journey in about an hour and nine minutes to enable it to retain its distance unchanged.

I have not here attempted to give the strict demonstration of the law which connects the mass of the central attracting body with the periodic time. Perhaps the following considerations, though they can hardly be said to amount to a proof, may suffice to explain the matter sufficiently. If the attraction of the Earth were to vanish, then the Moon would, of course, start off along a straight line, in continuation of the movement which it had at the moment when gravitation ceased. As a matter of fact, gravitation being in action, the Moon is continually forced to swerve from the straight line which it would otherwise pursue. It is, therefore, at each instant falling in, as it were, from the tangent towards the central attracting body. The distance through which it falls in an extremely short period of time, say, for example, a second, may be taken as the measure of the attraction to which it is exposed. Suppose that the mass of the attracting body be increased fourfold, then the distance through which the Moon must drop in one second would be increased fourfold. Look, then, at the adjoining figure, in which we have represented a small arc of the orbit and the tangent. The tangent line is the path in which the Moon would move but for the pull of gravity; the circular portion is the path in which it actually does move. If the two parts, T A, A B, be equal, then it is easy to show by calculation or by actual measurement that the part A P is one-fourth of B Q when these two lines are perpendicular to the tangent. If therefore the velocity of the planet be doubled, it will move over T B in the same time as it passes round over T A, but the fall to the orbit in the former case through B Q is four times the fall through A P. In other words, if the velocity be doubled, the attracting force must be increased fourfold. Having thus explained the law according to which the mass of the Earth affects the periodic time of the Moon, let us now consider the way in which the Moon's distance and its periodic time are connected together. Here we have no great difficulty, for we have already referred to the famous law of Kepler, which declares that for planets moving around the same primary the squares of the periodic times are proportional to the cubes of the mean distances. This principle will answer

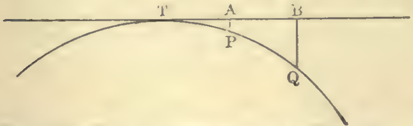


FIG. 15.—TO ILLUSTRATE THE EFFECT OF THE EARTH'S MASS ON THE PERIODIC TIME OF THE MOON.

every question regarding the periodic time which the Moon would have if its distance were altered to any other than its actual value. Thus if the distance were doubled, the cube of the distance would be increased eightfold. In this same measure the square of the periodic time would be altered; and it is easy to see that its increase would be in about the proportion of twenty-eight to ten. In other words, the Moon, instead of revolving round the Sun in 27·3 days, would require a period of no less than 76·4 days, if its distance were twice as great as it is at present. On the other hand, if we make an alteration in the periodic time, it will be equally easy, by an application of the third law of Kepler, to determine the mean distance at which the Moon shall pursue a circular orbit. For instance, let us suppose that the time was increased eight times, then the square of the time would be increased sixty-four times; and sixty-four being the cube of four, it follows that the distance of the Moon from the Earth would have to be increased fourfold.

Our study of the Moon has therefore shown that the periodic time in which a planet moves round its primary can be ascertained, provided we know the mass of the primary and the distance at which the revolution takes place. In fact, the three following quantities, namely, the distance, the periodic time, and the mass, are so connected that if any two of them be known, it becomes possible to determine the value of the third. Let us pursue, for example, the case already considered, in which we found that a body revolving at the distance of the Moon, around a central attracting mass equal in mass to the Sun, would accomplish a complete revolution in about sixty-nine minutes. Taking the same central body, let us now see what the distance must be at which a body revolving around the same attracting centre would complete its revolution in some other stated interval of time. The further off the body is, the longer, of course, the interval of time must be, the time and the distance being always connected by the relation that the square of the time varies proportionally to the cube of the distance. Let us imagine the body to be so remote that the periodic time, instead of being mere hours, shall have swollen into days. For instance, let the period be eighty-eight days; it is then easy to show by a calculation, the principle of which we have already explained, that the distance of the body must be about thirty-six millions of miles. This would, in fact, correspond to the actual movement around the Sun of the planet Mercury. If we take a somewhat more remote body, the planet Venus, of which the period is two hundred and twenty-seven days, a similar application of Kepler's law shows that the distance, in this case, must be sixty-seven millions of miles. Finally, let us suppose that the periodic time is 365·26 days, this being the time required for the Earth to perform its revolution. A repetition of the same process will show us that the distance of the body from the centre of attracting force must be ninety-three millions of miles. In other words, we have discovered the distance of the Sun. Here, then, we have been conducted to that method of ascertaining the Earth's distance from the Sun, which some mathematical astronomers maintain to be the best of all methods. Note exactly what our line of reasoning has been. First of all, from observations of the movement of the planet Mars, we have determined the relative masses of the Sun and the Earth, and we have thus shown that the mass of the luminary is about three hundred and twenty-four thousand times as great as the mass of the Earth. Once this relation has been accurately determined, the rest of the investigation is simple enough. The movements of the Moon have been determined with great accuracy. We know its distance and we know its periodic time; we therefore have the means of finding what the movements of the Moon should be if the mass of the Earth were other than it actually is. We can calculate what the movements of the Moon should have been had the Earth possessed that mass which we have already determined to be the mass of the Sun. Then, by Kepler's laws, we can ascertain to what distance the Moon should be moved, so that its revolution should be accomplished in precisely a year. In this way the Earth's distance from the Sun was determined. It should be noted that in this investigation the only uncertain element is the Sun's mass. This is ascertained, as we have already pointed out, from planetary observations, and as practical astronomy advances, and more and more observations are brought to bear upon the subject, there can be no doubt that our knowledge of the Sun's mass will constantly approximate to the absolute truth. This it is which makes the gravitational method one of such extreme value. It remains to show that even a slight error in the value of the Sun's mass will not affect, to any prejudicial extent, the value of the Sun's distance concluded from it.

What we have already stated involves the important law that if the periodic time be unaltered, the mass of the Sun will be proportioned to the cube of the distance between the Earth and the Sun. To take a particular illustration, suppose there were two systems, each consisting of a relatively small planet revolving around a central Sun, and that in one of these cases the distance of the planet from the Sun was double what it was in the other. If we further suppose the periodic times to be the same in the two cases, we can then infer from these facts alone that the mass of the attracting body in the larger orbit, being proportional to the cube of the distance, must be eight times as great as the mass of the body at the centre of the smaller orbit. If the diameters of the two orbits were in the proportion of ten to one, while the periodic times remained the same, then the masses of the two Suns would be in the proportion of one thousand to one. We have not attempted to set forth an accurate demonstration of this important law, but perhaps it will be possible to assign a reason for it in the following manner, which does not, however, profess to be an exact demonstration. Reverting to the first case in which the radius of one path was double the radius of the other, while the periodic times were the same, we can convince ourselves that the mass must be eight times as great in the large orbit as in the small one, by the following argument. As the periodic times are the same, the centrifugal force in the large orbit is double that in the small orbit, because the radius of the one is double that of the other. It therefore follows that the attracting force in one case must be double that in the other. But the attracting centre is twice as far away in the large orbit as in the small one, and since gravitation varies inversely as the square of the distance, the attracting body would have to be four times as massive in the one case as in the other, even to make the attractions equal. As, however, it is necessary for the attraction in the large orbit to be actually double the other, it follows that the mass at the centre of the large orbit must be not less than eight times as great as the mass at the centre of the small one.

Admitting the general truth that the mass is proportioned to the cube of the distance, we have the means of showing that an error in the mass will be comparatively unimportant in its effect on the distance. The easiest way of satisfying ourselves on this point will be to work out the calculation of the distance on two assumptions. First, suppose that the Sun's mass is three hundred and twenty-four thousand times the mass of the Earth, and that this gives a distance of 92,700,000 miles. Next suppose the mass of the Sun to be 325,000; in other words, that an error of about one three-hundredth part had been made in its determination; it is then easy to show that the cube of the distance must bear to the cube of 92,700,000 the same proportion which 325 bears to 324. Here is a little sum in arithmetic, and when it is worked out we find the amended distance to be about 92,800,000. But this is only 100,000 miles greater than the former value; that is, it differs from it by only one-thousandth part. In other words, an error of about one three-hundredth part in the adopted value of the Sun's mass, instead of propagating an increased error, as is often the case, is now reduced to about one-thousandth part. Before, however, we conclude the subject, there is an important matter to be considered.

I have already mentioned that the method of investigating the Solar distance now described is an approximate one, inasmuch as we have disregarded the mass of the Moon. It must be remembered that in considering the revolution of the Moon around the Earth, it has been necessary, in accordance with the well-known dynamical principle, to regard the mass of the Earth as augmented by the mass of the Moon. When this artifice has been adopted, we are enabled to make our computations just as if the mass of the satellite were infinitesimal, and as if it were revolving around the Earth fixed at the centre. It is now, however, necessary to take notice of the fact that the Moon's mass is not negligible, being really nearly one-eightieth part of the mass of the Earth; it will thus be seen, that the mass of the satellite bears a far greater proportion to the Earth than the Earth bears to the Sun. The difference is a significant one. In the consideration of the revolution of the Earth around the Sun we might regard our globe as having an imperceptible mass, and consider the whole mass as concentrated in the Sun. The periodic time of a particle of imperceptible weight revolving around the Sun at the mean distance of the Earth would be sensibly the same as that required by our own globe for its annual journey. Indeed, it is easy to show that the difference between the two would be less than a single minute. But the Moon bears such a considerable proportion to the Earth, that the periodic time in which a body of imperceptible weight would revolve around our globe at a distance equal to the mean distance of the Moon is distinctly different from that which the Moon requires.

It can be shown that, if the Moon were devoid of sensible mass, its distance remaining the same, its present periodic time would be increased by not less than four hours. Now it was clearly an imponderable Moon which we employed in our previous calculations. The whole argument was based on the mass of the Earth, and not on the mass of the Earth augmented by that of the Moon. We can, however, correct the periodic time of the actual Moon, and find what the periodic time of an infinitesimal Moon would be, provided we know the exact proportion between the mass of the Moon and the mass of the Earth. This is an element concerning which there is some uncertainty. We do not know it with an accuracy sufficiently great to enable us to deduce the Sun's mean distance precisely, in the way just described. Let us see, therefore, if we cannot employ some other and quite unexceptionable method to determine what the periodic time would be for an object which weighed, say, an ounce or a pound, or some other small amount, revolving in a circle at a distance from the centre of the Earth equal to the present mean distance of the Moon. Fortunately we have the means of solving this question with all required precision. It is well known that a body let fall from rest near the surface of this Earth will descend through a distance of 16·1 feet in the first second. This is an average result for the whole globe. If we want to express it with complete precision, we should say that a body at the Equator would fall from rest in one second through a space of 16·045 feet. The value of this quantity is open to no uncertainty that need be at present considered. It has been determined by careful research. No doubt it is quite true that direct experiments would not yield the requisite information very accurately. It would be difficult to measure to a fraction of an inch the precise distance through which a body drops in a second, or in any other definite interval of time. There are, however, artifices by which an electric spark, which lasts perhaps for the millionth part of a second, fixes, with the help of photography, the position of a moving body. Indeed, since Professor Boys has shown us how to take photographs of flying bullets, it is not difficult to conceive appliances of a similar kind which would measure the time occupied by a falling body. For the investigation of the velocity of projectiles chronoscopic instruments are, in fact, employed, which depend on the measurement of the distance through which a free body has fallen while the projectile is flying over a certain range. It then becomes possible to calculate the time that has elapsed, and thus to find the velocity with which the projectile was animated.

But by indirect methods we are enabled to determine with the highest degree of accuracy the distance through which a body falls in one second from rest. We employ for this purpose the oscillations of a pendulum, which in its simplest form is a weight suspended by a string from a fixed point. The great advantage which this instrument possesses for the measurement of gravitation depends on the circumstance that the time occupied by the pendulum in vibrating through a small arc is independent of the length of the arc of vibration. The time is solely connected with the length of the string and the intensity of gravitation. It is, indeed, possible to determine, with an extreme degree of precision, the time occupied by a vibration, for though it would be impracticable to make such a measurement if our attention was directed only to a single oscillation, yet when a large number of oscillations—say, for instance, a thousand—have been performed, and the total interval of time that they have required has been determined, we then ascertain with all possible precision the period of each single vibration. The accurate measurement of the length of the pendulum, though a matter of some difficulty, is still feasible; and knowing both its length and the periodic time, it becomes possible to determine the intensity of gravitation. Once this has been ascertained, we know, with all necessary accuracy, the precise distance through which a body would fall freely in one second at any latitude on our globe.

Suppose an ascent could be made to a point four thousand miles above the Earth—that is to say, to a distance which is as far above the Earth's surface as the centre is below it. At this point gravitation would be reduced to one-fourth of what it is at the Earth's surface; for, as we have already frequently had occasion to remark, the intensity of gravitation varies inversely as the square of the distance. A body which was released at this point, and allowed to drop down towards the Earth, would only fall one-fourth of the distance through which it would move in the same time if let fall near the Earth's surface. In other words, in the case which we have supposed, the body would drop four feet in the first second. Let us now assume a still greater elevation. Let us say, for instance, that the body is raised to a distance ten times as remote from the Earth's centre as the position which it originally occupied. Gravitation is then reduced to one-hundredth part of its original amount. The distance through which the object would fall is not, therefore, more than one-hundredth part of the distance through which it would fall in the same time on the Earth's surface. In other words, a body let fall from a height of thirty-six thousand miles above the Earth's surface would approach the Earth by only two inches in the course of the first second. Let us make one step more, and suppose that the experiment is repeated at a distance from the Earth's centre sixty times the Earth's radius, that is, at a height which indicates a point in the orbit of the Moon. According to the principles already laid down, the distance through which the body will there drop in one second must be diminished in the ratio of the square of the distance

—that is to say, it must be reduced to one three-thousand six-hundredth part of what it would be near the Earth's surface. It is easy to show that in this case the body would only fall in one second through the eighteenth part of an inch. We have thus found that a small body would fall in towards the Earth through the distance of one-eighteenth of an inch in the first second, if let fall from the distance of the Moon. In the course of our reasoning, I have, it is true, spoken of the body as if it were let fall from rest. The laws of motion, however, permit us to express the matter with a little more generality. I may state the result in this way:—That whatever be the movement of the body, it will, at the end of each second, be one-eighteenth of an inch nearer to the Earth than it would have been had terrestrial gravitation not acted upon it. From this consideration we can deduce the periodic time in which the body must revolve at the distance of the Moon. Draw a portion of a circle with a radius of two hundred and forty thousand miles, and draw a tangent. If the gravitation of the Earth were abolished, the body would move along the tangent. Owing, however, to the action of gravitation, the body is drawn down from the tangent into the circular curve. No doubt it would be impossible to make a figure with the true proportions, but the adjoining sketch (Fig. 16) may sufficiently represent the principle employed. Let A P be a small part of the circle struck with the centre O, and let A T be a part of the tangent. We have seen that the body will be drawn in one second towards the Earth through the eighteenth of an inch; let us, therefore, adjust the points P T in such a way that the distance P T shall be the eighteenth of an inch. It is quite plain that there can be only one position lying between the circle of two hundred and forty thousand miles radius and the adjacent tangent where the perpendicular interval between P and T will be exactly this eighteenth of an inch. We are able to measure the distance from A to T where this

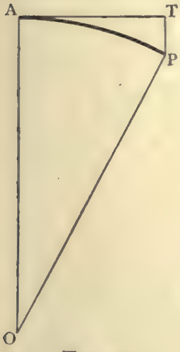


FIG. 16.

condition is fulfilled, and we find it to be about three-fifths of a mile.

If a circle be drawn with a radius of two hundred and forty thousand miles the curvature of the circle away from the tangent is necessarily very slow, owing to the great length of the radius. It can, however, be easily calculated, that at a distance of about three-fifths of a mile from the point of contact, the interval between the arc and the tangent is the eighteenth of an inch. At a distance greater than three-fifths of a mile the interval between the arc and the tangent is greater than that just mentioned, while at a less distance the interval is smaller. We have thus demonstrated that the movement of the body must be such that it performs every second three-fifths of a mile in its orbit; were it to move either faster or slower than this, it would not fall in towards the Earth through precisely that distance each second which the Earth's gravitation requires it to do. Thus the velocity of the body in its orbit is determined, and as the size of the orbit is known, it follows that the periodic time in which the body makes the circuit of its orbit is also determined. Here, then, we see that by means of pendulum experiments, combined with calculations based on the law of gravitation, we have actually been able to learn the periodic time of a body revolving in the orbit pursued by the Moon, while at the same time it had a mass inappreciable compared with the mass of the Earth.

I may take this opportunity of remarking that our sole method of determining the mass of the Moon with any accuracy is suggested by the argument which we have now been considering. I have shown that by the determination of the Earth's gravitation by means of pendulum experiments, it has been possible to find what the periodic time of the Moon would have been if our satellite had been devoid of mass. This is distinctly different from the periodic time which the Moon is actually observed to possess. The difference between these two periodic times can be accurately determined, and from it the mass of the Moon can be calculated.

One other observation should be made in connection with this subject. It will generally happen that the perturbations of the planets by which the Sun's mass is determined, involve not the mass of the Earth alone, but that mass augmented by the Moon. In this case it is plain that the earlier method which we employed, founded on the movement of the Moon alone, was more reliable than the later method, in which from pendulum experiments we were able to determine the periodic time of a body of insignificant mass revolving in the Moon's orbit.

There is now no difficulty in understanding the grounds on which so high a degree of importance is attached to this method of measuring the Sun's distance. If we knew for certain the Sun's mass, in comparison with that of the Earth, no other method could approach it; the only drawback consists in the fact that it depends on our theoretical investigations of planetary perturbations. We are always liable to be harassed by the reflection that there may have possibly been some disturbing agent at work among the bodies of the Solar System whose existence has not been recognised in our calculation. The uncertainty thus arising may legitimately be removed by the collection of determinations of the Sun's mass from all the different lines of astronomical research capable of yielding them. It is hardly possible that they can all be affected by any such systematic error as would lead to the deduction of a value invariably too high, or invariably too low, for the element in question. On these grounds it is that this method has been sometimes described as the method of the future, by which, after the lapse of a couple of centuries, the Sun's distance will be determined with a precision to which no other process can aspire.

It must, however, be admitted that every process of the kind here referred to lies open to a somewhat serious objection, from which some other methods of finding the Sun's distance are free. In the course of a long and elaborate calculation of a theoretical nature it is quite possible that considerations have been overlooked, or regarded as unimportant, which, if viewed in their true light, would attain sufficient magnitude to affect materially the conclusions arrived at. It seems, therefore desirable, that the methods by which our knowledge of the Sun's distance shall be ultimately obtained should be those which depend on what we may call surveying operations; that is to say, operations in which the distance results from measurements of angular intervals, supplemented by calculations of a perfectly definite character. In fact, as Sir D. Gill has well remarked, a true method should be such that, starting from the same figures, two accurate computers could not, by any possibility, arrive at different results. There is no doubt that, judged by this standard, the gravitational process lies open to serious objections, which may more than outweigh the advantages claimed for it on the ground that the accumulation of centuries of observation must lend increased precision to the result.

Before passing to the consideration of methods belonging to the other classes, I may mention another wholly distinct line of reasoning by which, from the phenomena of gravitation, a value of the Sun's distance can be obtained. This time we invoke the aid of the Moon to give information about the greater luminary. By the accumulation, during centuries, of lunar observations, the laws according to which the Moon pursues its round of the heavens can be ascertained with much precision. These movements are chiefly due to the attraction of the Earth, which, as the primary body, is chiefly involved in regulating them. But the Sun itself, attracting as it does both the Earth and the Moon, produces a disturbing effect on our satellite. If the Sun's attraction were equal on both bodies, then the relative movements of the Earth and the Moon would be unaffected. No doubt the Sun's attraction on the Earth and the Moon is nearly the same, but there is an inequality sufficient to make the movements of the Moon different from what they would otherwise be. These disturbances of the Moon's movement are well known to astronomers, and their numerical values can be elicited from a discussion of a lengthy series of lunar observations. Now it so happens that one particular effect of these perturbations upon the Moon's orbit is so directly connected with the distance of the Earth from the Sun that when lunar observation has disclosed its actual amount, the distance between the Earth and the Sun can be obtained by calculation. Here we have a method of discovering the distance of the Sun based upon assiduous lunar observation. The result given by it is, no doubt, of considerable value, but it falls somewhat short of that high standard of precision—to within one-thousandth part—which we have laid down as indispensable. The fact is, that the evaluation of the magnitude we want is here so mixed up with other elements that it cannot be discriminated from them so clearly as we could wish. It is, for instance, unavoidably associated with the measurement of the diameter of the Moon; and any error with which our determination of the diameter of the Moon may be affected would seriously impair the trustworthiness of the Solar distance yielded by the proposed method. We, therefore, only refer to it as possessing a high degree of interest from the recondite nature of the investigation involved. Notwithstanding its instructiveness from a theoretical point of view, it cannot compete, for practical purposes, with methods which depend on the more humble principles of simple surveying.

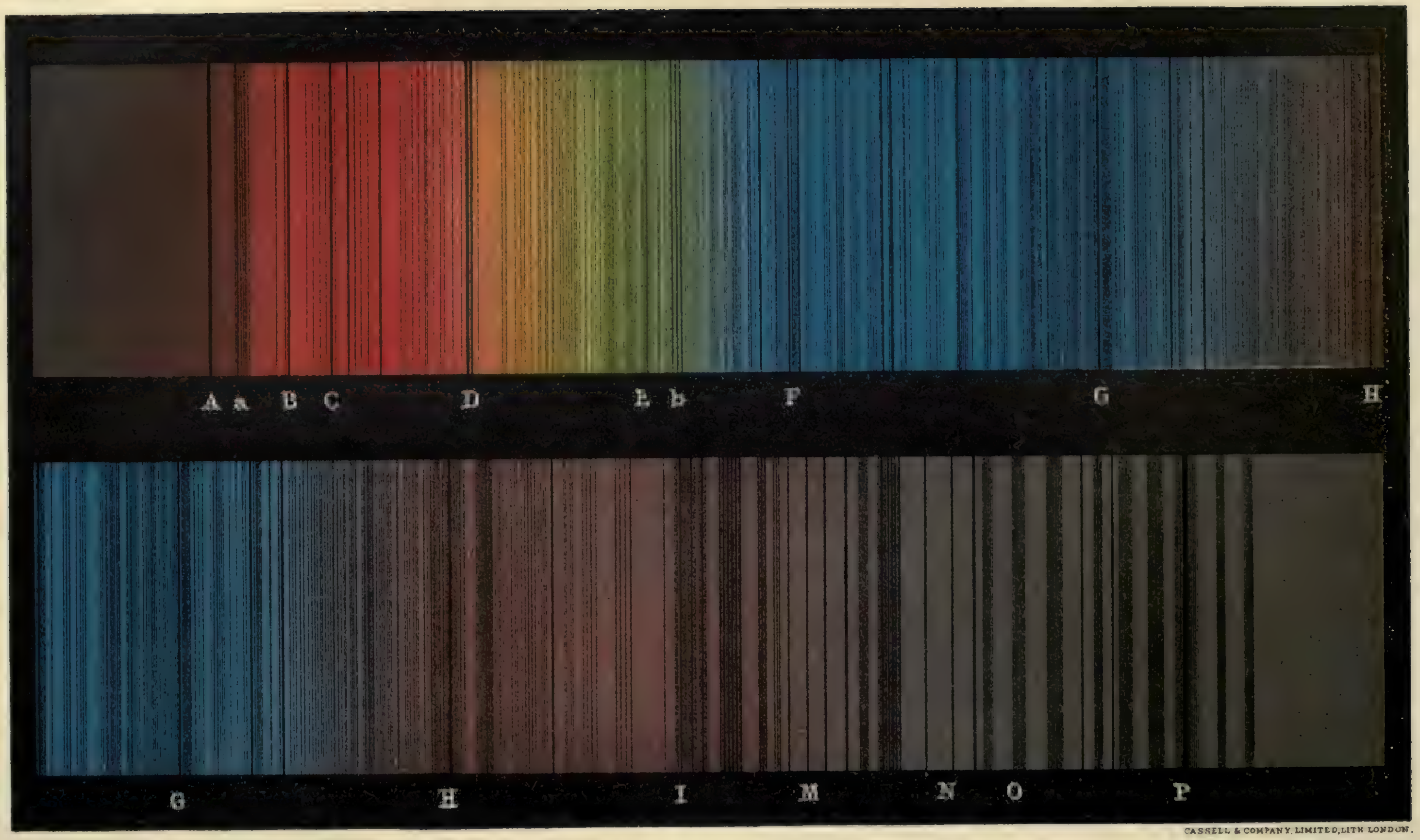
CHAPTER VI.

THE LIGHT OF THE SUN.

THE copiousness of the light radiated from the Sun always seems to me to be one of the most astonishing facts in Nature. Reflect, first of all, on the brilliance of a sunny day. Think how a hemisphere of the Earth is flooded with an illumination many thousands of times brighter than the brightest which could be produced by any conceivable artificial means. Think, too, that this splendid radiance has been transmitted to us from a globe at the tremendous distance of 93,000,000 miles. Think that the glorious orb which is the source of the light does not direct its beams more specially in our direction than in any other direction. Think that if you were standing on the Sun, and looked towards the Earth, the space that it occupied on the sky would not be more than the two-thousand-millionth part of the whole spherical surface. Think that all the sunlight which we receive, vast though its quantity be, is still only the two-thousand-millionth part of the total which the Sun dispenses. Think that this light has been diffused with a liberality quite as great as that with which it is now emitted, not merely for centuries or for thousands of years, but during the vast ages which geological research has opened up to us. Think that the forests which flourished at the time of the coal period were suffused with the same sunbeams as those which fall on our present woods and pastures. Think that the eyes of those great extinct animals whose fossil remains have been disinterred were constructed for the perception of that same sunlight which now gladdens the sight of the present inhabitants of the Earth. Think, in a word, that this abundant wealth of light has been freely lavished far and wide through space for a period compared with which the duration of the human race is but a moment. Think, too, that the oil in the lamp may not be replenished from any external source, but that the sunbeams have been maintained chiefly, if not entirely, by actions in the interior of the Solar globe. Surely, then, the origin of the light dispensed by the Sun presents a problem of the utmost interest and importance.

When we try to estimate the brightness of sunlight, we are in a difficulty for want of means of comparison. Let us consider some of the brightest lights known, and establish, if we can, some numerical relation between their brilliance and that of the Solar beams. In that very remarkable metallurgical process, the manufacture of Bessemer steel, the molten iron, as it flows white-hot from the furnace in which it has been fused, enters into a mighty vessel large enough to contain perhaps ten tons of the liquid metal. When the converter—for so the vessel is termed—is full, the characteristic part of this method of transforming iron into steel commences. Air, at high pressure, is driven forcibly into the bottom of the converter, and bubbles up through the molten mass. It might at first be thought that the act of blowing cold air through the heated iron must tend to cool the glowing liquid, but such is not the case. There is a certain quantity of the element carbon in the cast iron, and as the air bubbles through it the oxygen combines vigorously with the incandescent carbon, and heat is developed by this chemical union throughout the entire mass. The heat thus generated is far more than sufficient to neutralise whatever cooling effect might be the natural consequence of blowing volumes of cold air through the hot material. In fact, hot as the iron must have been when it was first run from the furnace into the converter, the temperature of the molten metal rises continuously during the twenty minutes or so that the blow continues. At last the experienced workman who is directing the operations detects, by the appearance of the flames emerging from the mouth of the converter, that the transformation has been accomplished. The steel is then found to be raised to a temperature so elevated that, when it is poured out into the mould, as it is in the next stage of the process, the fluid glows with a dazzling whiteness. No doubt the luminous arc of the

PLATE II.



CASSELL & COMPANY, LIMITED, LITH LONDON.

SOLAR SPECTRUM

electric lamp may be intrinsically brighter, but then it is only a very small surface. We cannot produce any glowing mass on a considerable scale so bright as the freshly-transformed molten steel. Let us, then, compare its brightness with the lustre of the Sun's surface. It has been shown by Professor Langley, to whose researches we owe so much in this department of science, that the intrinsic brilliance of the Sun is fully 5,000 times as great as if the whole surface were formed of the molten steel just issuing from the Bessemer converter.

The nature of light long involved questions about which there was considerable uncertainty. It is, however, now a thoroughly-established doctrine that the sensation of light is the consequence of waves or undulations which impinge upon the retina after transmission through that medium filling space which we call the ether. In subsequent parts of this volume we shall have occasion to refer to these ethereal undulations as the originating causes of the sensation of heat. We are now to discuss the effect recognised as the perception of light, which the same, or similar waves, produce on our optic nerves. Let us first endeavour to obtain some notion as to the characteristics of these waves. It must be remembered that the ether, whatever its actual nature may be, is something which eludes direct investigation by our senses. It cannot be regarded as a material substance, like the elementary bodies with which we are acquainted. It is a fluid of almost infinite subtlety and tenuity necessarily pervading all that space which contains visible objects, but not necessarily extending beyond certain limits. The importance of sound theoretical conceptions regarding ether is daily becoming more fully recognised, especially when we find that this medium is intimately connected with the manifestations of electricity, as well as of light and of heat. It is not easy to give visual representations of the actual nature of an ethereal undulation. There are, indeed, points with regard to the character of these waves which are still unsettled. But it will be indispensable for us to consider that primary element of the ethereal wave which we call its wave-length. What this is may be made clear by an illustration. Look at the waves of the sea as they roll in on the shore; then the interval between the crest of one wave and that of the one next succeeding is what we call the wave-length. This element may vary considerably; sometimes there will be a long swell, when the interval between one crest and the next may be many yards in length. All gradations are, however, met with, from undulations of this lengthy description down to little ripples lapping on the beach, where the interval between one crest and the next is only a foot or two, or even less. There is, however, another respect, besides that of length, in which waves of the sea differ from each other. We often speak of mighty waves, but we do not by such language refer to the length of the wave. The dimension implied is rather the greatest height of rise and fall in the wave—that is to say, the elevation of the crest in one wave above the hollow of that which precedes or which follows. This, it must be observed, is an element of a very different character from that which we describe as the wave-length. It is quite possible that waves which agree as to their length may differ very widely as to the heights through which they rise and fall. This latter element we speak of as the amplitude; so that the waves in the sea have at least the two characteristics of length and of amplitude. In a storm, when the waves are described as overwhelming in their vehemence, it is then amplitude which is the important element, the length of the waves being of comparatively little moment. Thus, there might be a sea-wave 100 yards in length where the amplitude was no more than a few inches; such an undulation would, of course, only be discernible by careful measurement. But with precisely the same interval of 100 yards between the crest of one wave and the next there might be waves with amplitudes of many feet or yards. The waves, in fact, might be vast billows, generated by a storm, though their length was no greater than the gentle undulations with which I have contrasted them. It is true that waves in the sea afford a somewhat unsatisfactory illustration of the character of waves in fluids of a more perfect description. But the phenomena of sea-waves will, at all events, suffice to explain the primary notions of wave-length and amplitude.

Sound is propagated by waves of vibration transmitted through the air. We can trace in the phenomena of sound a fundamental difference between wave-length and amplitude. The deepest bass tones of the organ arise from the longest sound-waves, but the loudness of the sound depends not on the length of the wave, but on its amplitude. In fact, the pitch of a musical note is defined entirely by its wave-length; while the loudness—which is, of course, a conception entirely distinct from that of the pitch—is measured by the amplitude of the wave. The greater the amplitude, the louder the note; the longer the wave-length, the lower the note. We thus completely define any musical note by specifying both its wave-length and its amplitude. It is, no doubt, true that there is a third quality of a musical note as rendered by any particular instrument. This quality is generally known by the word timbre. It has, however, been ascertained that the distinctive qualities of musical sounds implied by this word are due to the intermixture of harmonic overtones with the primary note sounded. Thus a certain note rendered on the violin differs from the same note sounded by the human voice, even if the intensity or loudness of the two should be deemed the same. This distinction arises from the circumstance that certain feeble harmonics are in each case added to the fundamental note. These harmonics consist of undulations, the wave-lengths of which bear simple fractional relations to the wave-length of the fundamental tone. It seems almost impossible to produce a perfectly pure note—that is to say, one entirely devoid of timbre imparted by the presence of some harmonics. In the case of the violin the harmonics added to the fundamental note are not the same as those added by the human voice. The same notes sounded by the two consequently differ as to timbre. But if we could refine away these adhering harmonics, so as to obtain each note in a perfectly pure form, then the character of the note would be completely defined by its wave-length and amplitude.

This discussion of the nature of musical sounds will prepare us for the interpretation of certain analogous phenomena presented in the case of light. This also consists of undulations; only the medium in which they are performed is not that atmosphere which is agitated by audible undulations. It is undulations of the ether which constitute what we call light. Perhaps it is more correct to say that when certain undulations enter our eyes, the sensations into which we translate them form what we mean by the word light. And now we see the importance of the distinction which we have drawn between the length of an undulation and its amplitude. The waves in ether, as in other fluids, are characterised both by wave-length and by amplitude. The wave-length indicates the colour of the light, while the amplitude expresses its brightness. This is a fundamental point, and on its development depends the interpretation of luminous phenomena generally.

The first point to be noticed with regard to the light emitted by the Sun is its wonderfully complex character. A ray of sunlight appears to us so simple in its pure beauty that it is at first rather difficult to realise the highly composite nature really belonging to it. Our first examination of the character of sunlight must be made by decomposing a beam into the elements of which it is constituted. The simplest way of effecting this decomposition is by means of a glass prism in the manner represented in the adjoining figure. When a ray of white sunlight falls on a prism (Fig. 17), it is found on emergence to be deflected from its original direction, and the colours are differently acted on. Thus the red rays will be bent less than those of a greenish hue, and these, again, less than the blues or the violets. The prism therefore provides

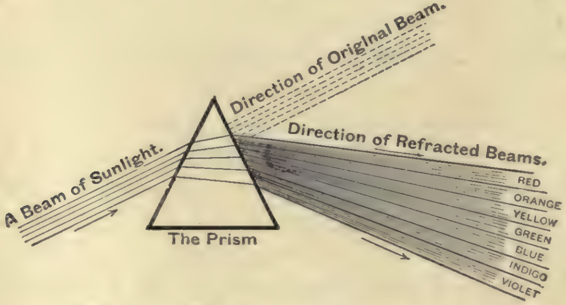


FIG. 17.—THE DECOMPOSITION OF LIGHT.

us with a means of analysing a source of compound light into its constituents. Suppose that it is formed by the union of red, green, and violet beams. Then our eyes are incompetent to detect the several ingredients in their composite form; but as the beam passes through the prism, the analysis is instantly effected. The violet is bent most, the red least, while the green is directed to an intermediate position. To form a beam of sunlight all the colours of the rainbow—red, orange, yellow, green, blue, indigo, and violet—are intimately blended. It is by their union that the pure white light was produced; and the prism decomposes this white beam into its several constituents. The different colours possess different refrangibilities, and are therefore differently acted on by the prism; so that the single beams spread out into a fan-shaped stream, the several hues representing the different rays of the fan. It is on this principle that we produce the Solar spectrum. Its nature demonstrates the composite character of sunlight. In each region of the spectrum a certain characteristic hue is perceived, and the various hues in the different parts of the coloured band blend into each other by almost insensible gradations. To each one of them belongs a certain wave-length, and we might

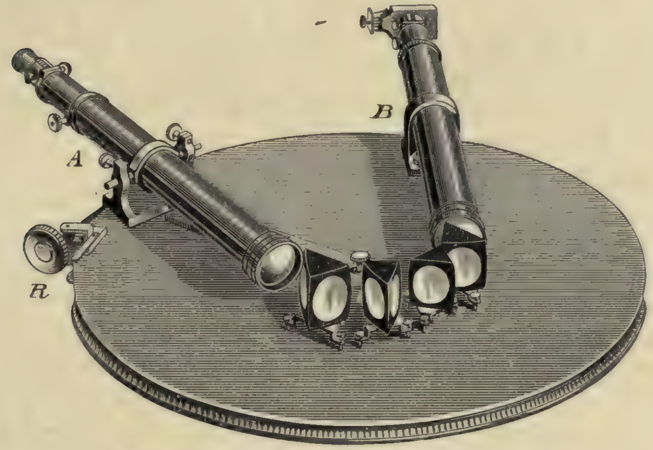


Fig. 18.—The Four-Prism Spectroscope.

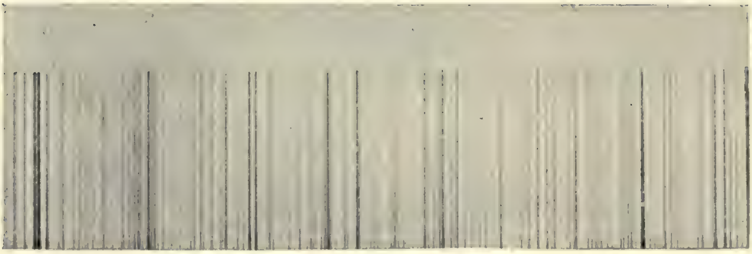
therefore say that a beam of sunlight is composed by the union of waves whose lengths have almost every possible value lying between certain limits. The spectrum may be likened to the sound generated by a device for striking every note on the piano simultaneously, but with somewhat varying intensities. And as the loudness of a musical note depends upon the amplitude of the atmospheric vibration by which it is generated, so in the case of light the brightness of each particular hue depends entirely upon the amplitude of the corresponding ethereal vibration. In the study of the spectrum produced by the light from the Sun or from other celestial bodies, we have a most interesting department of research; in fact, it is hardly too much to say that some of the most important developments of astronomy made in modern times are connected with the examination of light according to the principles here laid down. Besides the means of resolving composite light into its constituents afforded by the prism, there is a more elaborate process which, because it affords far more delicate presentations of the Solar and other spectra, has lately tended to supersede the prism. This new process depends upon the reflection of light from the surface of a mirror on which extremely fine parallel lines have been engraved. The effect of the "grating," as this instrument is called, is to produce a spectrum of such purity that its minute features can be examined more perfectly than by the older process. The importance of this new method is so great that I must devote special consideration to the remarkable results to which it has led.

Let us suppose the spectrum of the Sun has been obtained either by a prism, as in the earlier method of observing, or preferably by a grating such as has just been described. It is found that the coloured strip is interrupted by the presence of a multitude of fine dark transverse lines. With every increase in the power of the apparatus, or in the purity of the spectrum, the lines become more and more numerous. They are frequently observed to be disposed in pairs. Sometimes they are associated in well-defined groups, and sometimes in striking combinations repeated with a species of rhythm over a considerable length of the spectrum. From the well-known fact that the lines invariably reappear with precisely the same features when the proper appliances are used and the necessary precautions taken, it is obvious that they must be intrinsically connected with the light itself, and cannot be attributed merely to the instruments with which the observations have been made. The latest improvements in the art of producing Solar spectra have enabled us to depict these lines by the help of photography, and extremely successful results have been obtained by several astronomers. In the adjoining plate we are enabled to give some representations of photographic maps of parts of the Solar spectrum obtained by Mr. George Higgs, of Liverpool. I may refer to Mr. A. C. Ranyard's excellent description of this notable achievement in photographic astronomy given in *Knowledge* of September 1st, 1890.

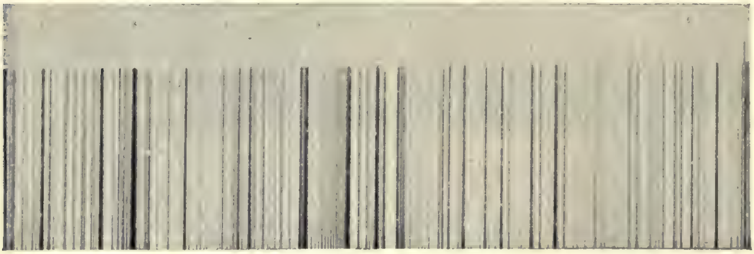
It was in the year 1880 that Professor Henry A. Rowland completed a machine by which he was enabled to produce screws with a perfection of mechanical precision never previously attained. I remember seeing at Baltimore, in 1884, the exquisite apparatus at work which Professor Rowland employed in the production of his celebrated gratings. By means of one of his wonderfully accurate screws he was able to rule a grating on a polished surface of speculum metal with as many as 43,000 lines to the inch. The machine by which this most delicate operation was effected was situated in a cellar, so as to avoid, as far as possible, such errors as would be introduced by variations of temperature, and many ingenious mechanical devices were introduced to minimise the irregularities resulting from unavoidable defects in the mechanism. At last Professor Rowland succeeded in obtaining ruled gratings where the errors in the lines did not exceed the one-hundred-thousandth of an inch. A spectroscope furnished with one of these pieces of exquisite workmanship exhibited spectra with a previously unrivalled purity and brilliance. The most effective gratings appear to be those ruled on a concave mirror. Glorious indeed is the display of the Solar spectrum exhibited by one of Professor Rowland's concave gratings, six inches in diameter, and twenty-one and a half feet radius. On the occasion of my visit to Baltimore I enjoyed the privilege of witnessing this splendid spectacle, and as I moved from one side of the chamber to the other with the eye-piece, with which the reflection from the grating was viewed, the lines of the Solar spectrum passed before my view in glorious procession.

Mr. George Higgs, whose beautiful photographs are shown in the plate, has made use of one of Professor Rowland's concave gratings similar to that already referred to, but of lesser dimensions. It is four inches in diameter, and has a radius of about ten feet two inches. In order to render the plates intelligible it will be necessary for me to give first some explanation of the nomenclature employed for the designation of the different objects in the Solar spectrum. The dark lines which stand out most conspicuously are known by the letters A, B, C, D, E, F, G, H. Certain other lines of greater or less importance are indicated by small letters of the alphabet. But besides these methods of designation, there is another, of a very different character, which permits us to identify any particular line of the spectrum by a certain wave-length. It will be understood, from what we have already said, that to each part of the spectrum corresponds a certain wave-length, and consequently the position of each line in the spectrum is precisely indicated when we know the wave-length to which it corresponds. Extreme care has been taken to determine, with all attainable accuracy, certain standard wave-lengths in the Solar spectrum, from which the wave-lengths of other lines can be ascertained by measurement. To take one of the most important cases, there is the D_2 line, which is one of the pair formerly designated by the symbol D. The wave-length of this line has been recently investigated in an elaborate manner by Mr. Lewis Bell, of Johns Hopkins University. It is convenient to express such small magnitudes as those which are here involved by what are called tenth-metres, each of these units being one ten-thousand-millionth of a metre. It may be observed that the tenth-metre means simply a metre divided into the number of parts indicated by unity followed by ten ciphers. Similarly, of course, an eighth-metre would be that part of a metre found by dividing it by the number denoted by unity followed by nine ciphers. Mr. Bell has found that the wave-length of light possessing the same refrangibility as

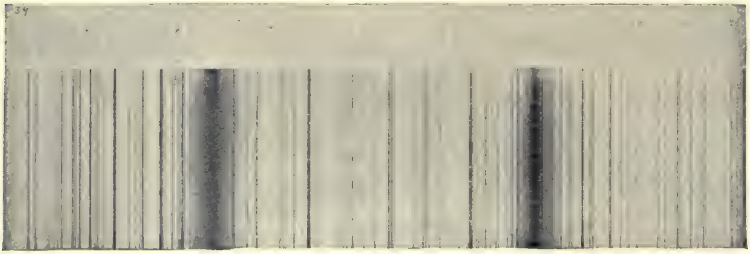
PLATE III.



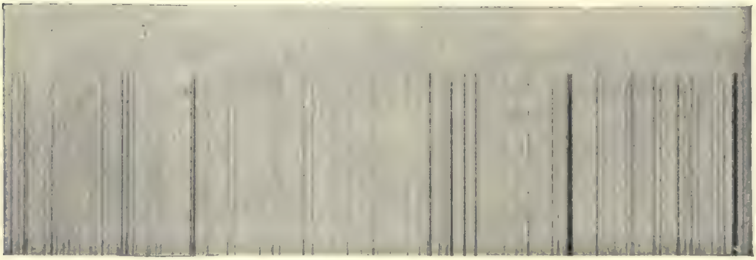
Region between P and O.



Region L and K.



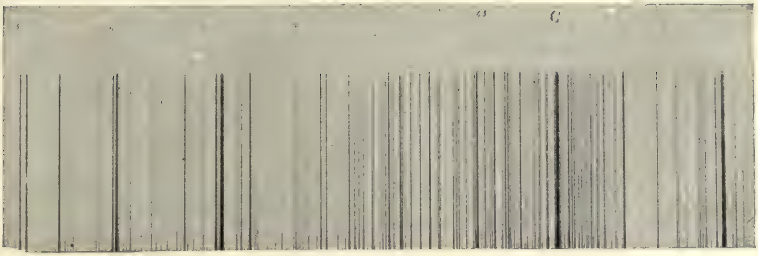
The H and K Lines.



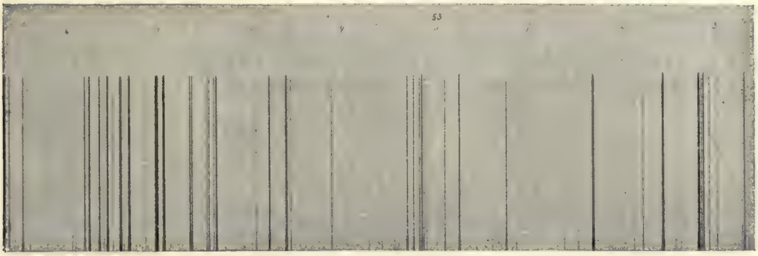
Region between H and h.

PHOTOGRAPH OF SOLAR SPECTRA (*Higgs*).

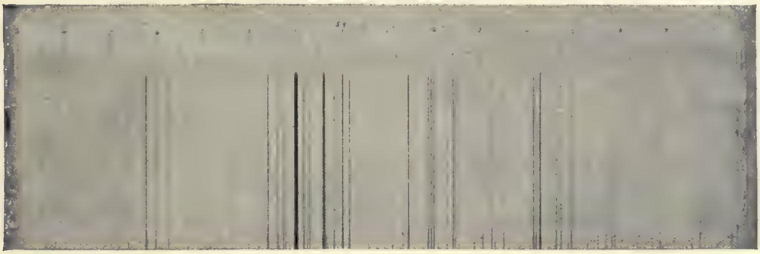
PLATE IV.



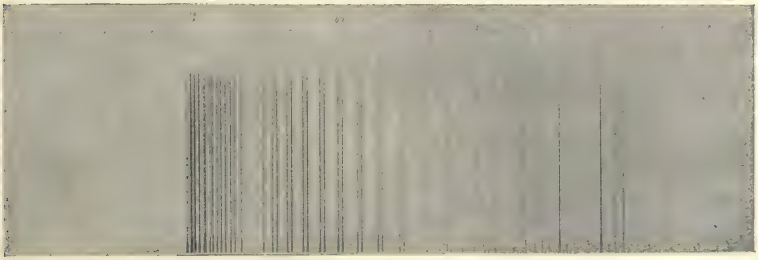
Great G Group.



The E Group and 1,474 Lines.



The D and Rainband Group.



Great B Group.

PHOTOGRAPH OF SOLAR SPECTRA (*Higgs*).

the line D_2 is 5890·188 tenth-metres. From this and similar determinations the wave-lengths of intermediate lines have been ascertained. I do not, however, enter into further details on this somewhat technical matter; suffice it to say that by specifying the corresponding wave-length we can indicate the position of any line in the spectrum in a perfectly definite manner. It should be remarked that with the present refined methods of examining the spectrum, features are disclosed which could not have been detected by the less perfect methods formerly available. Thus many of the lines which were formerly regarded as single, are now seen to be composed of double lines very close together. Indeed, they are often decomposed into systems of far greater complexity. Thus the first section of Mr. Higgs's map represents what is known as the great B group, which appears on the left-hand side as a dense cluster of lines. In small instruments these lines cannot be separated. The resolution of them here brought about is something like the effect sometimes exercised by a powerful telescope on a nebulous object submitted to its examination, whereby it is resolved into a myriad of stars. In like manner the B group, when seen under sufficient instrumental power, is decomposed into a multitude of lines. The observer will notice the way in which certain of the lines are grouped in pairs, somewhat suggesting the rulings in a copy-book. These marvellously symmetrical lines are very impressive when seen with Rowland's great spectroscope. The second section of Mr. Higgs's photograph is very beautiful, and at once brings into evidence the great power of the method employed. This is the part of the spectrum best known, and those who have only been accustomed to instruments of comparatively small power will be astonished to perceive how highly magnified the line known as D is in the present picture. It is not only that the famous sodium line has been split into a pair—with that phenomenon every spectroscopic observer is, of course, familiar—but between two members of the pair three others show quite conspicuously, besides at least five or six of a fainter character. The third section represents the group known as E, in which the remarkable coronal line known by the number 1474 may be seen. The fourth section of Mr. Higgs's map, exhibiting the spectrum in the neighbourhood of the great G group, is a very instructive one, from the extraordinary profusion with which the lines are displayed over this part of the spectrum. The fifth, sixth, seventh, and eighth sections are of interest, inasmuch as they lie chiefly in that part of the spectrum which is wholly beyond the range of the human eye. For it must be noted that in addition to ethereal waves of lengths adapted to excite the organs of vision, the Sun pours forth multitudes of other undulations which, though they may fall on the eye, yet are powerless to produce a sense of light. Our eyes possess nerves responsive only to particular vibrations, and insensible to vibrations of any other type. But it so happens that, though we are unable to see those waves of light which appertain to a part of the spectrum beyond the violet, yet it is these particular waves which operate with peculiar efficiency on photographic plates. Indeed, it is not too much to say that the light chiefly effective in the taking of photographs is the light which we cannot perceive under any circumstances whatever. Want of brightness is not here concerned; it is the quality of the light that tells. We therefore view portraits of these regions of the spectrum with particular interest, inasmuch as the photographer is not here merely depicting objects which we could otherwise see. His aid in the study of this part of the spectrum is absolutely indispensable. We could never have learned the existence of these lines without photographic assistance.

But by far the most important information which the spectroscope affords us about the Sun relates to the materials of which the great luminary is composed. One of the greatest extensions given within recent times to our knowledge of the universe lies in the interpretation of the multitude of dark lines which cross the Solar spectrum. It will be necessary to enter into this subject with some little detail.

For the sake of illustration I shall consider the element iron, and I shall suppose that between two iron poles a powerful electric current is made to pass. The light thus produced is viewed through a spectroscope, and the slit with which the instrument is furnished is made to transmit a narrow slice of the radiations emanating from the glowing arc between the iron points. This light passes through the prisms, or equivalent dispersive apparatus of the spectroscope, and thus the rays are decomposed into their constituents. It then becomes evident that light from the vapour of iron consists of a large number of distinct rays blended together. The appearance of the spectrum is not that of a coloured band of light, as in the case of the Sun. The spectrum of the incandescent iron vapour presents a multitude of bright lines, each lying apart from the others, and each coloured with the hue appropriate to that part of the spectrum in which it is situated. These lines are quite characteristic of iron. Supposing that the specimens of iron with which our investigations have been made are perfectly pure, then the lines produced belong specially to iron, and are a sure indication of the presence of that metal. If the poles had been made of any other metal—of copper, for instance—then the spectrum would consist also of bright lines, but these lines would occupy totally different positions, and be grouped in quite a different manner from the lines of the iron spectrum.

We might extend the same considerations to other metals, and indeed to all the known elements. Each of them, when sufficiently heated, yields incandescent vapour, and emits light which the prism decomposes into a group of bright lines characteristic of that particular substance. The adjoining figure (Fig. 19) shows the spectra of a number of elements, with the Solar spectrum for comparison. The same process is also applicable in the study of gaseous spectra. Let us suppose that through a capillary tube containing a small quantity of rarefied gas—hydrogen, for example—a current of electricity be passed, it will glow in such a way that the light can be submitted to the analysing powers of the spectroscope. The gas thus treated will yield a group of lines so characteristic of its particular nature that whenever these lines are observed the presence of that particular gas is indicated. The element we have named is specially remarkable from the circumstance that it is indicated in the photographs of the spectra of many of the brightest stars. Indeed, it should be noticed that many of the lines belonging to this element exist in that non-visible part of the spectrum only revealed by photography. The most striking features of the spectrum of hydrogen would be entirely lost if we depended upon mere visual observation.

The lines which we have been considering are produced by the application of heat to the elements under examination. They are accordingly bright lines projected on a dark background. But the problem presented by the Sun is how to explain the presence of black lines on a light background. The discovery of the connection between the bright lines of one spectrum and the dark lines of another was indeed a notable event in modern science. It was found that when the Solar spectrum was placed side by side with the spectrum of iron obtained by the electric spark, the correspondence between the bright lines of one

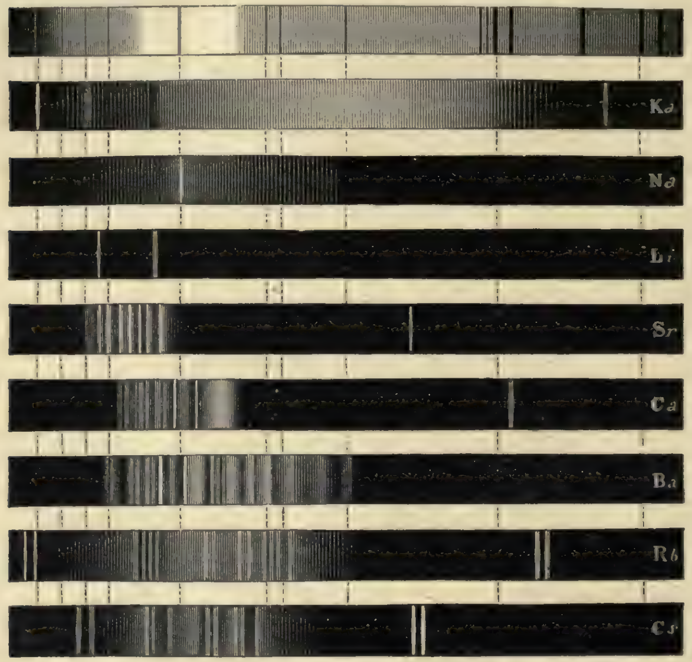


Fig. 19.—Spectra of Various Elements.
K*a*, Potassium; N*a*, Sodium; L*i*, Lithium; S*r*, Strontium; C*a*, Calcium; B*a*, Barium; R*b*, Rubidium; C*s*, Caesium.

spectrum and certain dark lines in the other was such as could not possibly be accidental. It would, indeed, be sufficiently surprising if even one conspicuous bright line in the iron spectrum coincided exactly with a prominent dark line in the Solar spectrum. Should it appear that a second bright line in the iron spectrum also tallied with another of the dark lines, the coincidence would be thought still more striking. Indeed, it would be almost impossible to deny that there must be some physical reason for the relation which such coincidences implied. When it further appeared that the bright lines in the iron spectrum tallied with the dark lines of the Solar spectrum, not merely in twos and threes, but even in hundreds, or, indeed, as Professor Rowland has found, in no fewer than two thousand instances, then it became a practical certainty that there must be some connection between the spectrum of iron and the dark lines of the Solar spectrum.

Though the existence of some physical link between the spectra was thus established, yet it did not at first seem at all plain what the nature of that link was. The difficulty now to be considered involves the explanation of the fact that one spectrum consists of bright and the other of dark lines. Naturally the first suggestion, and, as we shall presently see the true one, is that there must be iron in the Sun. If, however, this iron were heated in the Sun in the same way as it was heated by the electric spark, it might be thought that the indications of its presence should be made by bright lines rather than by dark lines. It presently appeared, however, that while the correspondence between the two spectra was indeed an unquestionable indication of the existence of iron in the Sun, yet that the dark lines indicated a remarkable circumstance in Solar physics. Their origin was rightly attributed to the fact that, outside the brightly glowing part of the Sun, there is an atmosphere hot enough to maintain iron in actual vapour, although less hot than the highly-heated photosphere from which the light of the Sun is mainly dispensed. From this photosphere a flood of light is poured forth; but before it can reach the Earth it has to traverse the outer atmosphere, which contains, relatively non-luminous iron vapour. And now comes in the operation of one of the profoundest of Nature's laws. The iron vapour in the exterior Solar atmosphere intercepts certain rays in the light passing outwards from the interior. This absorption, however, is not indiscriminately exercised; in fact, the great majority of the rays of light which seek to pass through the vaporous iron are permitted to pursue their journey unmolested. But certain rays are almost wholly arrested; in fact, the atmosphere of iron vapour is nearly opaque to these particular rays, while it may be said to be transparent to rays of all other descriptions. And now for the singular circumstance characterising this phenomenon. It has been found that those rays which the iron vapour absorbs have precisely the same refrangibility as the particular rays of light which iron vapour itself gives out when sufficiently heated. We may illustrate the principle here involved by reference to an analogous fact in connection with the atmospheric undulations of sound. The strings of a piano will often respond to notes struck in their vicinity, but it will be invariably perceived that the particular strings set vibrating in the piano are the very strings which themselves, when struck in the ordinary way, give out the particular notes to which they are found responding. There is a well-known experiment—very easily tried with the piano—which illustrates this point in a striking manner. Suppose that a good voice sings a certain note vigorously to the open piano, intoning at the same time a vowel-sound such as A; then if the ear be held close to the instrument, the strings of the piano will be distinctly found to echo back a feeble A. If, however, the same note be sounded on the vowel O, then in the echo returned by the piano, though still of the same pitch, the vowel-sound O will be distinctly perceptible. The explanation of this phenomenon is very simple. For the production of a vowel-sound certain subsidiary harmonics have to be introduced by the voice in addition to the fundamental note. Indeed, it is by the combination of the different harmonics that the different vowel-sounds can be produced. On the reception of the sound by the piano each string will vibrate if it receives any undulation of the same periodic time as that in which it is itself adapted to vibrate. On the other hand, those strings whose oscillations do not happen to agree with any of the undulations communicated in the composite note, are not called into action. The consequence is that the several strings which have been appealed to respond with proportionate intensity, while the other strings remain dumb. We thus see that the vibrating parts of a musical instrument possess the power of absorbing vibrations which are in unison with their particular periods, while they permit other vibrations to pass unnoticed. In fact, the piano-strings have the power of selecting from a mixed volume of sounds passing across them those particular vibrations with which they synchronise, while to the other vibrations they are perfectly neutral. This is an illustration of what occurs in the passage of light through any medium. Instead, however, of the atmospheric vibrations which are concerned in the production of sounds, we have in the case of light to consider the ethereal vibrations by which radiation is effected. The molecules of an elementary body in a state of vapour have certain periods in which, so to speak, they are timed to oscillate. If acted upon by undulations in harmony with any of the vibrations which the molecules themselves are competent to perform, some of the energy of these passing vibrations will be absorbed by the molecules, and they will swing with correspondingly increased vehemence. If, however, the vibrations passing the molecules do not happen to be in tune with such movements as the molecules are capable of executing, the vibrations pass on their way with undiminished energy. We are now able to understand how the vapour of iron in the cooler outer atmosphere of the Sun stops and appropriates to itself the energy of those particular undulations which are in sympathy with the molecular periods of iron. The consequence is that such undulations being arrested light of those particular kinds suffers a heavy tax on its outward passage. It follows that in the corresponding parts of the spectrum we see black lines, or lines which appear black in contrast to the vivid brilliance of the illumination on either side of them. Lest there should be any misapprehension on the subject, I may remark that the absorption is never perfect, a considerable quantity of light being found even in the darkest lines, notwithstanding the distinctness given to them by contrast.

Provided with this interpretation of the significance of the dark lines in the Solar spectrum, we see how invaluable the spectroscope becomes as an instrument of research. In the first place, it has demonstrated the presence of iron in the Sun. Note the special importance of this addition to our knowledge, for it would have been utterly hopeless for us by any other means at present known to have learned what are the actual materials of any body outside the Earth. We might have conjectured, no doubt, that an element so abundant as iron is in the Earth would not have been found wanting elsewhere; but still a conjecture only it would have remained. It is true that we occasionally receive on the Earth a meteorite which has dropped from the sky, and as these meteorites generally contain iron, it would not have been unreasonable to conclude that iron must have some existence beyond the limits of our own Earth. But such conjectural knowledge would have been widely different from the accurate information which the spectroscope has yielded. It has demonstrated, with a certainty hardly inferior to that afforded by any other province of astronomical research, that the element iron is certainly present in the great luminary.

I have taken iron as an illustration, but it will, of course, be understood that the comparison may also be made between the spectra of other elements and that of the Sun. Many astronomers have worked at this subject, but it will generally be admitted that as to the character of the Solar spectrum, and as to the identification of the elements present in our great luminary, the work of Rowland, already referred to, is the most comprehensive that has yet been undertaken. In the Johns Hopkins University circular for April, 1891, Professor Rowland describes the progress that he had made up to that time. He tells us that he had compared with the Solar spectrum the spectra of all known elements except a few gaseous ones, and a few others too rare to have been as yet comprised in the research. These comparisons have been made by photography, in the vast region of the spectrum between the extreme ultra-violet and the D line of sodium. Eye-observations for comparison have also been made throughout the entire length of many elementary spectra. Professor Rowland makes the very interesting remark that the greater number of the lines in the elaborate photographic map of the Solar spectrum have been already identified with terrestrial elements. No fewer than thirty-five bodies known on the Earth have thus been detected in the Sun. Among them may be mentioned the following:—Iron (2,000 lines or more); nickel, manganese, cobalt, carbon (200 lines or more); calcium (75 lines or more); palladium; magnesium (20 lines or more); sodium (11 lines); silicon; strontium; barium; aluminium (4 lines); zinc; copper (2 lines); silver (2 lines); tin; lead (1 line); potassium (1 line). Eight terrestrial elements are classed by Professor Rowland as doubtfully present in the Sun. The best-known of them is platinum, the others being very rare metals. The same distinguished physicist also gives a list of fifteen elements of which he cannot detect any lines in the Solar spectrum. The names enumerated include antimony, arsenic, nitrogen, gold, mercury, phosphorus, and sulphur. He adds, however, the caution that it would be quite erroneous to conclude, from the fact that the lines of these several elements have not actually been detected in the Sun, that therefore the elements named must be absent. Indeed, the remarkable conclusion has been arrived at that if the whole Earth were heated to the temperature of the Sun, its spectrum would probably resemble very closely that of the Sun.

It must not, however, be inferred that all the lines without exception exhibited in the Solar spectrum are to be attributed to absorption in the Solar atmosphere. Before the rays of light reach our instruments, they have to traverse, at the end of their long journey, the strata of the Earth's atmosphere, hundreds of miles in thickness. In the course of that progress they undergo selective absorption from our air, which has the effect of adding fresh lines and characteristics to the so-called Solar spectrum. Many of these lines are due to the vapour of water suspended in the atmosphere, and there is also good evidence to show that the oxygen which is so large and important a constituent in air is responsible for several of them. M. Janssen, having reason to believe that the well-known dark lines usually denominated A and B were produced by atmospheric oxygen, essayed to test this belief in a very energetic manner. It was, of course, possible that even if these lines were due to oxygen, they might originate through the presence of that element in the Solar surroundings. To decide this question, M. Janssen ascended Mont Blanc in July, 1890. At the elevation thus reached, the bands in question were found to be so much weakened in intensity that it seemed impossible to doubt that they would disappear altogether if the influence of the atmosphere could be entirely eliminated. It should, of course, be remarked that this does not necessarily prove the absence of oxygen from the Sun; all that it strictly proves, is that certain known features in the spectrum are due to atmospheric oxygen.

Modern research is continually making us acquainted with the marvellous complexity and interest of the Solar radiation. The ether which fills space transmits to us from the great luminary a volume of vibrations of the most varied description. Such is the wondrous subtlety of the ethereal fluid that it can tremble with waves one ten-millionth part of an inch in length, while propagating similar vibrations which are feet or yards or miles, or even thousands of miles, from crest to crest. With the full import of these undulations we are only gradually becoming acquainted; our eyes, beautifully adapted though they be, can only show us a few octaves of vibrations in one particular region. Photographic plates have the power of disclosing shorter vibrations than those appealing to any nerves of vision; and in other indirect ways we are coming to know of the existence of some of the longer undulations. It is true that we have no organs of sense to which ethereal undulations, feet or yards or miles in length, can directly appeal; nevertheless we have the best reason for knowing that such vibrations do actually exist, and by certain experimental methods we have recently been impressed with their importance. The splendid experiments of Hertz have disclosed methods of examining undulations of this long type. How wonderful some of these may be we can conjecture from the fact that, if our eyes were provided with nerves which would respond to such vibrations, we should be able to see through stone walls with the same facility as we can now see through glass. We must remember that transparency is a purely relative idea. The undulations which suit our eyes are of such a length that they can pass through glass; but the undulations of photographic light do not pass perfectly through even the clearest glass; this material not being quite transparent to such short waves. On the other hand, such waves of light as suit our eyes cannot pass through the stone walls, which some of the very long waves Hertz has disclosed to us can traverse quite easily. It sometimes seems that in the contemplation of the Sun's radiation our eyes, even with all their perfections, are just as imperfect in interpreting fully what the Sun dispenses, as would be the ears of a man who could only hear the beating of a drum when attempting to listen to an orchestral performance of a beautiful symphony. The solemn tones of the organ, the exquisite delicacy of the stringed instruments, would be alike inaudible; all that he could hear would be the tones of a solitary instrument. Almost equally imperfect are our eyes for the interpretation of those marvellous radiations which the Sun sheds abroad with such prodigality. His beams really contain a vast key-board of vibrations of which our eyes can only disclose to us a couple of octaves. Here and there within the mighty compass of the instrument we are able to devise a photographic plate, or a Hertzian resonator, to astonish us with a few additional octaves, but of the full volume of the harmony we are almost entirely ignorant. When we think that the Sun pours forth its radiations with such prodigality that our Earth is only able to intercept the two-thousand-millionth part of the whole; when we think that the other Suns in space, to the number of many millions, are each of them simultaneously filling the ether with their orchestra of vibrations; when we think of the marvellous delicacy with which that medium transmits all these vibrations —we begin to realise the majesty of the words "Let there be light."

CHAPTER VII.

ECLIPSES.

IT may seem strange to say that a great part of our knowledge of Solar phenomena has been derived from the assistance of the Moon. The Sun-spots, no doubt, can be observed and studied without any help from our satellite; but for our knowledge of the appendages outside the photosphere of the Sun, we are primarily indebted to assistance rendered by the Moon. In fact, it was by such help that the existence of those flames which leap from the Solar surface, and of the glorious halo known as the corona, were discovered. It is true that our modern resources have latterly enabled us to dispense with the assistance of the Moon in observing Solar prominences; indeed, so much is this the case that when a total eclipse now occurs we devote comparatively little attention to the examination of the prominences, inasmuch as they can be studied almost as satisfactorily in full sunshine. We are still, however, wholly dependent on the assistance which the Moon offers for displaying the splendour of the corona. It is only when the dark globe of the attendant world intrudes between the Earth and the Sun, intercepting all direct light, that the glories of that marvellous fringe by which the Sun is surrounded are revealed.

It has always struck me as one of the most remarkable coincidences in Nature that the apparent diameter of the Sun should happen to be so nearly equal to the apparent diameter of the Moon. No doubt the former of these bodies is about four hundred times as remote from us as the latter, but then it also happens that the diameter of the Sun is about four hundred times as great as that of the Moon; and that being so, the apparent diameter of the two objects, or—to speak a little more accurately—the angles which the lunar and solar diameters subtend at the eye of the terrestrial observer are very nearly equal. The consequence is, that when the Moon happens to be placed directly between the observer and the Sun, the Solar light, as a rule, is just completely intercepted. The near equality of their apparent diameters will be obvious from the fact that while the Moon is sometimes able to produce a total eclipse, it is at other times only capable of effecting what is known as an annular eclipse; in which latter case the Moon, even when centrally posited, is still entirely surrounded by a margin of brilliant Solar surface. The truth is, that the distances both of the Moon and of the Sun from the observer are continually fluctuating within certain narrow limits. If the Moon happens to pass across the Sun at a time when the satellite is at a considerably greater distance, and the Sun somewhat closer than usual, then we have the phenomenon of an annular eclipse. On other occasions, however, when the Moon and the Sun are at their average distances, the apparent size of the Moon is sufficiently great to hide the Sun completely, and thus afford one of those rare opportunities so greatly appreciated by the practical astronomer.

An eclipse of the Moon takes place when our satellite plunges into the shadow of the Earth. The circumstances under which a lunar eclipse happens will be sufficiently illustrated by the accompanying figure. Here S is the Sun, and T the Earth, and the shadow B O B′ is evidently such that an object entering into its dark cone will be entirely deprived of all light from the Sun. In its revolution around the Earth the Moon passes in close proximity to this shadow every time that it is full. Usually speaking, however, owing to the inclination of its path, the position of the full Moon lies above or below this cone, and in

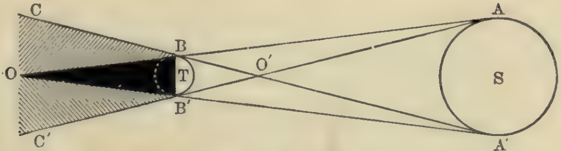


FIG. 20.—THE EARTH'S SHADOW.

such cases there is no eclipse. It sometimes, however, happens that the Moon plunges partly, or it may be wholly, into the shadow; and when this occurs, the direct radiation from the Sun is intercepted, and the Moon is said to be eclipsed. This spectacle we describe as a total eclipse or a partial eclipse, according as the whole of the Moon or only a part thereof becomes obscured by the deprivation of sunlight. The various details are illustrated in Fig. 21, adopted from the author's Atlas of Astronomy.

We may here notice the very curious circumstance that even when the Moon is so deeply immersed in the Earth's shadow that all direct light from the Sun is intercepted, yet it frequently happens that at the very climax of the eclipse the lunar globe is observed to be glowing with a remarkable copper-coloured lustre. So bright indeed is this light that the particular features on the surface of our satellite can not unfrequently be detected. Its presence in such cases is due to the fact that rays do not in general pursue absolutely straight lines when passing through the Earth's atmosphere. It is quite obvious, from the figure we have already given, that if the lines in which the light travels were, all of them, without exception, perfectly straight, then no light whatever could enter the shadow, and a total eclipse of the Moon would necessarily imply that the Moon had become totally invisible. But the great atmosphere encompassing our globe modifies the rays of light which pass through it by bending them into the shadow, so that a multitude of the beams from the Sun which happen just to graze the Earth will in the course of their journey be deflected inward into the shadow, and prevent an object in that shadow from becoming entirely invisible.

It is now not difficult to account for the peculiar copper-coloured hue with which the Moon sometimes glows during a total eclipse. (See adjoining plate, Fig. 21.) Those ruddy rays of light to which the illumination is due have traversed a vast thickness of our atmosphere; not only have they become bent by the passage, but another important effect has been produced —they have changed their hue. It is the property of our atmosphere to exercise a specific absorption upon rays of light transmitted through it. Of all the colours of the rainbow—red,

PLATE V.

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APPEARANCE OF THE MOON DURING ECLIPSE. *(Guillemin.)*

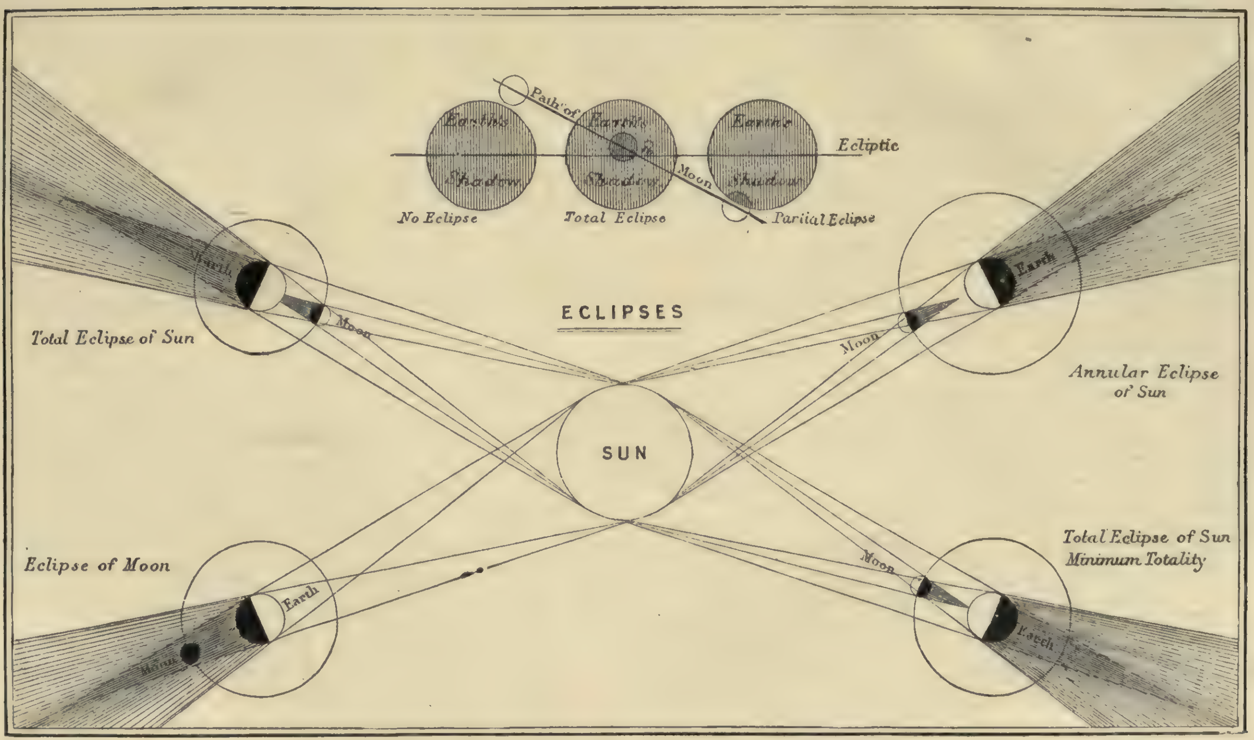


FIG. 21.—THE DIFFERENT TYPES OF ECLIPSE.

orange, yellow, green, blue, indigo, violet—the rays last named have the least penetrative power. They are stopped, so to speak, in their passage through the air, and the consequence is that after a beam of light which was primitively white has traversed a sufficient thickness of air it becomes sensibly red, because the blue rays have been abstracted from it by absorption. We witness this phenomenon at sunset, where the ruddy hues of the setting Sun require no other explanation than that which has just been given. Nor will there now be any difficulty in understanding why the peculiar hue presented by the totally-eclipsed Moon should be ruddy or copper-coloured. The beams by which the Moon is illuminated have had to make an atmospheric journey twice as long as that imposed upon the beams of the setting Sun. In the course of this long journey they are reddened even more than those of the setting Sun, and hence the remarkable hue imparted by them to the Moon.

This interesting phenomenon is, however, subject to striking variations. The cause to which these variations are due will be at once comprehended when we reflect that the light which arrives at the totally-eclipsed Moon only does so after a long course through the terrestrial atmosphere. It may often happen that the zone of air at the margin of the Earth's disc, as it would be seen from the Moon, is heavily charged with clouds. In this case the rays, seeking to pass through such dense parts of the atmosphere, would be so much obstructed that they might perhaps be entirely stopped. These are the rays which would otherwise have entered the cone of shadow and illuminated the Moon with its ruddy hue. Their interception by the clouds deprives the Moon of any illumination whatever, and brings about veritably a total eclipse. An observer on the Moon during a total eclipse would find that the dark disc presented by the globe of the Earth was surrounded by a narrow luminous band of a deep ruddy colour, not by any means uniform in lustre even though the Earth were centrally interposed between the lunar observer and the Sun; for under all circumstances the terrestrial atmosphere is considerably charged with clouds; and where such clouds exist in sufficient abundance, the ruddy margin of light must be correspondingly interfered with.

One of the simplest propositions which can be demonstrated with regard to eclipses is that those of the Sun must be more frequent than those of the Moon. That this is so will be manifest from a consideration of the adjoining figure. In this diagram S represents the Sun and T the Earth, and the two tangent lines to both globes are represented as meeting at the point O. An eclipse of the Moon will occur whenever our satellite or any part of it dips into the black shadow on the left of the Earth between O and T. On the other hand, an eclipse of the Sun of some kind, and visible from some part of the Earth, must be the consequence of a plunge of the Moon into any part of the cone between the Earth and the Sun. This latter part of the cone is larger than the former, and accordingly in the course of its revolutions around the Earth the Moon will more frequently dip into that part of the cone between the Earth and the Sun than into the part formed by the shadow of the Earth. Hence it follows that Solar eclipses must, on the whole, occur with greater frequency than Lunar eclipses. At first this might appear to be somewhat at variance with ordinary experience, but a little consideration will show that such is not the case. We do not assert that at any particular locality more Solar than Lunar eclipses will be visible; this is, in fact, not the case. It must be remembered that whenever a Lunar eclipse occurs, it must be visible to all the dwellers on that hemisphere of the Earth which happens to be turned towards the Moon at the time; for since a Lunar eclipse simply means the cutting off of light from the whole or part of our satellite, it follows that from whatever point on the terrestrial hemisphere the satellite was looked for, an obscuration would be immediately apparent. A Solar eclipse, however, merely implying the interposition of the Moon or a part of it between the Earth and the Sun, is not by any means necessarily visible over the whole of the hemisphere. It would,

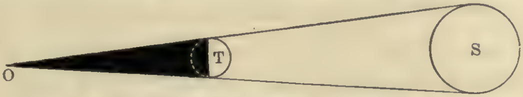


FIG. 22.—TO ILLUSTRATE THE FREQUENCY OF SOLAR ECLIPSES.

in fact, only be visible from that limited part of it whence observers would see the disc of the Moon projected in whole or in part against the disc of the Sun. We can demonstrate that, though as a matter of fact there are more Solar than Lunar eclipses every century, yet that to the inhabitants of any particular locality on the Earth Lunar eclipses present themselves with greater frequency than Solar. In explanation of this fact it is only necessary to observe that the section of the Earth's shadow at the distance of the Moon is broader than the apparent diameter of the Sun. Indeed, at the distance of the Moon the area occupied by the shadow of the Earth has an angular diameter nearly four times as great as the apparent diameter of the Sun. If the Moon enters the smaller of these areas, she eclipses the Sun; if she enters the larger area, she is eclipsed herself. It is quite obvious that, one of these areas being very much larger than the other, the entry of the Moon into it will be a phenomenon of greater frequency than the entry of the Moon into the smaller region. The consequence is that to an observer remaining always at one spot of the globe, a Lunar eclipse will be a more frequent phenomenon than a Solar eclipse; while to an observer who roams over the world, so that no eclipse of any kind whatever escapes him, Solar eclipses will be more numerous than Lunar.

As to the laws of the recurrence of eclipses, a few general principles may be laid down, with the remark, however, that the accurate determination of the time at which a particular eclipse will occur is a question always involving a long and tedious calculation. The performance of such laborious work is undertaken by the computers of the Nautical Almanac, to which invaluable publication reference may be made for the times of the various phenomena connected with the different descriptions of eclipse. It may, however, be noted that long before learned men were acquainted with the true nature of the movements of the heavenly bodies—long before, for instance, they had come to understand that this Earth revolves round the Sun, rather than the Sun round the Earth, it had been found possible to predict eclipses with a considerable degree of accuracy. The only source of such predictions was a principle, which long experience had confirmed, regulating the sequence of such phenomena. It was found that a certain period of eighteen years and eleven days was so remarkably related to eclipses, that when all such occurrences during one cycle had been carefully recorded, then a similar set of eclipses were unfailingly repeated at corresponding dates in the following cycle of the same length. Although there may be little or no obvious relation between the solar and lunar eclipses that are observed this year, and those which were observed last year, or those which will be observed next year, yet we may generally assert that eighteen years and eleven days after one eclipse has taken place another eclipse may be expected.

We first speak of eclipses of the Moon; and for the sake of illustration I choose a total eclipse of the Moon, visible at Greenwich, which occurred on June 11th, 1862. Counting on eighteen years and eleven days from the date just named brings us to June 22nd, 1880; and, accordingly, on that date there was again a total eclipse of the Moon. As another illustration, taken in the same year, we note from the Nautical Almanac that there was an eclipse of the Moon on December 5th, 1862. Adding on again this noteworthy period of eighteen years and eleven days, we are brought to the date of December 16th, 1880, which was marked by a repetition of the same phenomenon. Such was the astuteness of the ancient astronomers that they had detected the law just illustrated from the comparison and careful observation of long series of eclipses. It will be noted that this principle affords us the means of predicting the recurrence of eclipses—not, indeed, with infallible certainty, but by a rule sufficiently accurate for practical purposes. Caution must, however, be observed in its application. If, for instance, an eclipse be a very small one, it does not certainly follow that another eclipse will be perceived after eighteen years and eleven days; and as a counterpart to this caution we may add that a small partial eclipse may occasionally occur, even though no similar phenomenon took place at that date eighteen years and eleven days previously.

We must now explain how it is that this period happens to possess the remarkable property we have attributed to it. It will be remembered that a lunar eclipse can, as a matter of course, only take place at full Moon ; it is also necessary that at the same time the satellite be in or close to its node—that is to say, the point where the orbit of the Moon passes through the plane of the Earth's circuit. It appears from the nature of the celestial movements that in the period of eighteen years and eleven days nineteen apparent revolutions of the Sun take place with reference to the Moon's nodes. In very nearly the same period two hundred and twenty-three lunations are accomplished—the lunation being, of course, the average interval between one full moon and the next. Here, then, the remarkable coincidence is to be noted, that the Sun returns, after nineteen revolutions, to its original position with regard to the Moon's nodes just in the time that the Moon occupies in accomplishing two hundred and twenty-three lunations. To make the circumstances clear, it should be explained that in a year—or, rather, in a little less than a year—after the occurrence of a lunar eclipse the Sun will again have reached the point opposite to the node; but the Moon will not then be found there, nor at the second, nor at the third complete revolution of the Sun relatively to the node, will the Moon be in the position proper for an eclipse ; it will be too far from the node. But since, after nineteen entire revolutions of the Sun with regard to the node, just two hundred and twenty-three lunations have been completed, then the conditions required for an eclipse of the Moon are reproduced. The time required for these two hundred and twenty-three lunations is this period of eighteen years and eleven days. Hence we infer that whenever the conditions are such that the Moon is full when at its node, then eighteen years and eleven days from that time the Moon will be again near its node at the time of the full, and a lunar eclipse will again be likely to occur.

It will be understood that this rule is only to be regarded as approximate, nor will it apply over very great stretches of time, because the numerical relation between the lunations and the revolution of the Sun with regard to the Moon's nodes given above is not true enough to be relied upon when great accuracy is required. Neither will the rule suffice to give any accurate idea of the details of lunar eclipses, such as the time of their commencement or their duration, or any of the other particulars which the Nautical Almanac sets forth in such abundant detail for the edification of astronomers. To ascertain such matters very careful computations have to be made; they depend upon a knowledge of the motions of the Moon, acquired by long-continued observation.

It likewise follows that this same period of eighteen years and eleven days will conduct from the occurrence of a New Moon at the node to the next similar occasion. Now, a New Moon at the node implies, quite necessarily, a solar eclipse, and hence we can assert that a solar eclipse will usually be followed in eighteen years and eleven days by another phenomenon of the like description. It is not, however, possible to employ our knowledge of this period for the prediction of solar eclipses with the same completeness as in the case of lunar eclipses. It is defective in one point, indispensable to the thorough usefulness of such predictions. It cannot tell us from what localities on the Earth the eclipse will be visible. The distinction between the two cases must be remembered :—The lunar eclipse, being visible from a whole hemisphere of the Earth, must be discernible by every inhabitant of that hemisphere, granted certain other conditions as to weather and the like. But as the solar eclipse is only a very partial phenomenon, we cannot infer that an eclipse will be visible at Greenwich to-day because a similar phenomenon was seen there eighteen years and eleven days ago.

All that can generally be stated is, that if on a certain day an eclipse of the Sun has been visible anywhere, then after the lapse of eighteen years and eleven days an eclipse of the Sun must be visible somewhere on the Earth. Practical astronomers can indeed make their computations of solar eclipses with any required degree of accuracy. They can not only tell what eclipses are to occur this year or next, but they can calculate the details of the similar phenomena that will happen any time within the next two thousand years, or that have happened any time during the past two thousand years. This period of eighteen years and eleven days, to which we have referred as being so important in the theory of eclipses, is often known as the Saros.

It will be proper to mention that there is another period, known as the Cycle of Meton, which plays a very important part in the several relations between the Sun and the Moon. It can be shown that the exact length of two hundred and thirty-five lunations is 6939·69 days. It also appears, by simple multiplication, that nineteen years, reckoned each at 365·25 days, amount to 6939·75 days. As the former of these periods is only about an hour and a half shorter than the latter, we are justified in saying that nineteen years is almost exactly equal to two hundred and thirty-five lunations. From the almanac-maker's point of view the Cycle of Meton has a distinct importance. It tells him that in nineteen years from any date the Moon will have made a number of complete revolutions around the Sun, and therefore the age of the Moon, on any particular day of the year, will be reproduced on that day nineteen years, or, more precisely, after an interval of 6939·6 days. Thus, for instance, on the day that these lines are being penned—that is, on the 28th of March, 1893—the Moon is ten days old. If we refer back 6939·6 days, we come to the 28th of March, 1874. In this interval the exact number of two hundred and thirty-five lunations have been performed. The age of the Moon will therefore have been the same as on the same day of the year nineteen years later.

There is a certain practical consequence of the Cycle of Meton which I may mention. The times of high water and of low water at any given locality depend chiefly on the Moon's age. No doubt there are certain refinements which have to be attended to when very exact time-tables are required. For our present purpose, however, we may take it for granted that the age of the Moon and the hour of high water at a given place stand in a relation of sufficient constancy for many practical purposes. Thus, for instance, I note that whenever the Moon is full the tides are high on Dublin Bar at ten o'clock in the morning. No doubt the actual time of high water may be a quarter of an hour or half an hour more or less, but the rule is often accurate enough. If, therefore, on any particular date the Moon is full and the tide is high about ten o'clock in the morning, then on that day nineteen years (6939·6 days), the Moon will also be full, and high water will also be found at about ten o'clock. Similarly, when the Moon is at the first quarter the high tide at

Dublin occurs at about four o'clock. Within certain limits the same is the hour of high water at the same locality whenever the Moon is at the quarter. Hence it follows that in nineteen years after the tide has been high at four o'clock it will again be high at the same hour. We learn moreover from this principle that if a tide-table has been constructed for any port so as to be right for every day during nineteen years, and if the table be copied out again, moving every date forward by 6939·69 days, we thus obtain a tide-table accurate enough for many purposes, for a further period of nineteen years. The Cycle of Meton also enables us to determine the hour of rising or of setting of the Moon on any day in any year, when once a table has been prepared for nineteen consecutive years. For the hour at which the Moon must rise on any given day of the year will depend on the age of the Moon. The age of the Moon on any day will, however, by the principle that we have explained, also be the age of the Moon on that day nineteen years. It follows that the hour of Moon-rise must be the same in the two cases. It should, however, be observed that the Moon does not move exactly in the ecliptic; its plane is inclined to it at an angle of five degrees, and some small variation in the hour of the Moon's rising or setting will thus arise. The number which each year bears in the Cycle of Meton is called the Golden Number. In 1890 the Golden Number was 1, being the commencement of a new cycle taking place in that year.

We shall have much to say about the revelations made by a total eclipse of the Sun when we discuss the corona. Here we shall only refer to the remarkable effects witnessed on the Earth when such an event takes place. Plate V. has been reproduced from the drawing in which Professor Piazzi Smyth depicted the eclipse of 1851 as seen in Norway, and with his description of it we conclude the present chapter:—"To understand the scene more fully, the reader must fancy himself on a small rocky island, on a mountainous coast, the weather calm, and the sky, at the beginning of the eclipse, seven-tenths covered with thin and bright cirro-strati clouds. As the eclipse approaches, the clouds gradually darken, the rays of the Sun are no longer able to penetrate through and through, and drench them in living light as before; but, as with clouds on an evening sky, they become darker than the background on which they are projected. The air becomes sensibly colder, the clouds darker, and the whole atmosphere murkier. From moment to moment, as the totality approaches, the cold and the darkness advance apace; and there is something peculiarly awful and terribly convincing in the two different senses so entirely coinciding in their indications of an unprecedented fact being in course of accomplishment. Suddenly, and apparently without any warning—so immensely greater are its effects than those of anything else that had before occurred—the totality supervenes and darkness comes down. The shadow of the Moon must evidently have a very well-defined termination; and those who have seen a large eclipse, or even an annular one, have no idea of what a 'total' eclipse is like. Then started into view lurid lights and forms, as, on the extinction of the candles, a phantasmagoric picture, before unnoticed, may be made to appear prominently imposing in a darkened room. This was the most striking point of the whole phenomenon, and was precisely that which made the Norse peasants about us fly with precipitation, and hide themselves for their lives. Darkness was everywhere, in heaven and on earth, except where along the north-eastern horizon a narrow strip of unclouded sky presented a low burning tone of colour, and where some distant snow-covered mountains, beyond the range of the Moon's shadow, reflected the faint monochromatic light of the Sun partially eclipsed, and exhibited all the detail of their structure, the light and shade markings on their precipitous sides, with an apparently supernatural distinctness. After a little time the eyes seemed to get accustomed to the darkness, and the looming forms of objects close by could be discerned, all of them exhibiting a dull green hue, seeming to have exhaled their natural colours, and to have taken this particular one, merely by force of the red colour in the north. Life and animation seemed, indeed, to have now departed from everything around; and we could hardly but fear, against our reason, that if such a state of things were to last much longer, some dreadful calamity must happen to us also, while the lurid horizon northward appeared so like the gleams of departing light

PLATE VI.

CASSELL & COMPANY, LIMITED, LITH LONDON.

DARKNESS DURING TOTAL ECLIPSE OF 28th JULY, 1851, AT
BUE ISLAND, NORWAY.

(From Sketches by Prof. Piazzi Smyth.)

in some of the grandest of the works of Martin and Danby that one could not at the time, and in that presence, but believe, in spite of their alleged extravagances, that Nature has opened up to the constant contemplation of their mind's eyes some of those magnificent revelations of power and glory which others can only get a hasty glimpse of on occasions such as these."

CHAPTER VIII.

SUN-SPOTS.

THE surface of the Sun frequently exhibits certain markings which it is our purpose to describe and to investigate in this chapter, leaving the photosphere, the prominences, and the corona for subsequent consideration. They may be understood from Fig. 23, which represents the appearance of the Sun when the Sun-spots are presented at a time of maximum development.

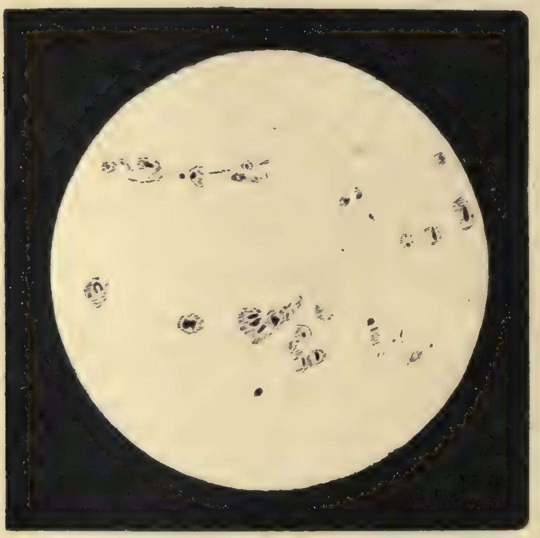


Fig. 23.—The Sun at the time of Maximum Sun-spots.

In Fig. 24 we represent what may be described as an ideal Sun-spot. It is dark in the central region, and surrounding that "umbra," as it is called, is a margin known as the "penumbra," from the outer edge of which the face of the luminary again resumes its brilliancy. The spots are very varied in their appearance; sometimes the Sun is almost entirely free from them, at other times they are abundantly present. It will thus be evident that these objects are not permanent characteristics of the solar globe, in the same sense as the craters on the Moon or the continents on Mars are permanent features of the globes to which they belong. The Sun-spots are variable features exhibited by that envelope of glowing clouds surrounding the Sun which we call the photosphere. The want of permanence in Sun-spots is immediately connected with the fact that they have their origin among those solar clouds which possess little more durability than do the clouds in our own atmosphere. We have, indeed, the best reasons for knowing that changes on the most gigantic scale, and manifested with the utmost vehemence, are incessantly in progress in the photosphere. Thus it sometimes happens that the glowing clouds are parted asunder, and a glimpse is

Fig. 24.—A Sun-spot showing Penumbra.

afforded through the opening into the solar interior. It appears almost certain that the inside of the Sun has much less power of radiating light than is possessed by the exterior, and consequently such peeps of it as we obtain disclose a darker surface than that of the photosphere.

The general character of a Sun-spot is excellently illustrated in the accompanying photograph of the Sun's surface, which was taken by M. Janssen at Meudon on the 10th June, 1887, with a refracting telescope five inches in aperture. I am indebted to the kindness of M. Janssen for permission to reproduce it in Fig. 28.

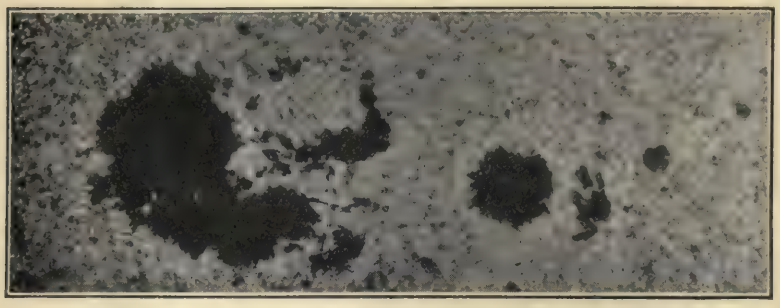


Fig. 25.—Photograph of Sun-spot, June 1st, 1881. (*Janssen.*)

The scale of the picture may be judged of from the fact that if the entire image of the Sun had been photographed, its diameter would be about three-quarters of a metre. The study of this picture will make it manifest that a Sun-spot is really an opening through the brilliant photosphere, by which we get a view of the dark solar interior. The fleecy-looking materials around are luminous clouds which float suspended in the solar atmosphere; and where the clouds are drawn away, there a spot is formed. Our present photograph affords an excellent illustration of the penumbra. This is the shaded region which intervenes between the intensely dark interior and the brilliant surface of the luminary. The extended cluster of small spots will also be noticed. The general surface of the Sun as here represented is, besides, marked over with black dots, which are really no more than the interstices between the glowing clouds. The granular structure of the Sun is, indeed, a very noticeable feature, the dark interior being visible between the adjacent grains. It is, however, quite plain that the large spot is distinctly different from a mere enlargement of one of the small spots, or pores, which abound all over the surface. There seems to be some special agent in operation by which the clouds are apparently repelled from the large opening. Over the surface of the plate we notice several places where the definition seems to be imperfect; it is known that these do not arise either from casual imperfections of the plate or from accidents in the development; they plainly owe their origin to some veritable cause in the Sun itself, nor shall we find it hard to explain what that must be. The solar clouds are, as we know, in rapid motion; and there are other circumstances connected

Fig. 26.—Spots near the Sun's Limb.

with those solar prominences to be subsequently described, which make it clear that the velocities with which these glowing gases are animated must be exceedingly great. It can, indeed, be shown that certain parts of the photosphere are moving with speeds so high that even in the hundredth part of a second —which is about the duration of the exposure of this plate— the movements of the clouds are sufficiently great to produce the observed indistinctness. We therefore infer that the blurred parts of the picture are those in which such rapid movement was in progress that even in the hundredth part of a second perceptible effects were produced. As an illustration of a Sun-spot where the penumbra is absent I give a picture taken by Dr. Janssen on June 1st, 1881 (Fig. 25).

It is clear that a rapid interchange must be going on between the external parts and the internal parts of the Sun. The highly-heated exterior regions are radiating forth heat into space: as they lose that heat, they become cool, and having become cool, they contract, and consequently must descend. This is in conformity with the ordinary laws of heat, which must be as true in the Sun as they are on the Earth. It therefore follows that at certain parts of the Sun's surface there must be a down-rush of vast quantities of cooled gas from above, and a corresponding welling upwards of heated materials from beneath, which sustain the radiation. It would seem that a certain type of spot, such as that represented on Fig. 25, forms what we may describe as a sink on the Sun's surface, where vast masses of solar vapour are descending which have parted with their heat by radiation. In the comparatively cool current thus suggested, clouds of glowing vapour could not exist, and consequently we can see directly into the interior, so that the appearance of a dark spot is presented. Let it not, however, be inferred that because this spot seems to be dark in its representation in the photograph


Fig. 27.—A Sun-spot.

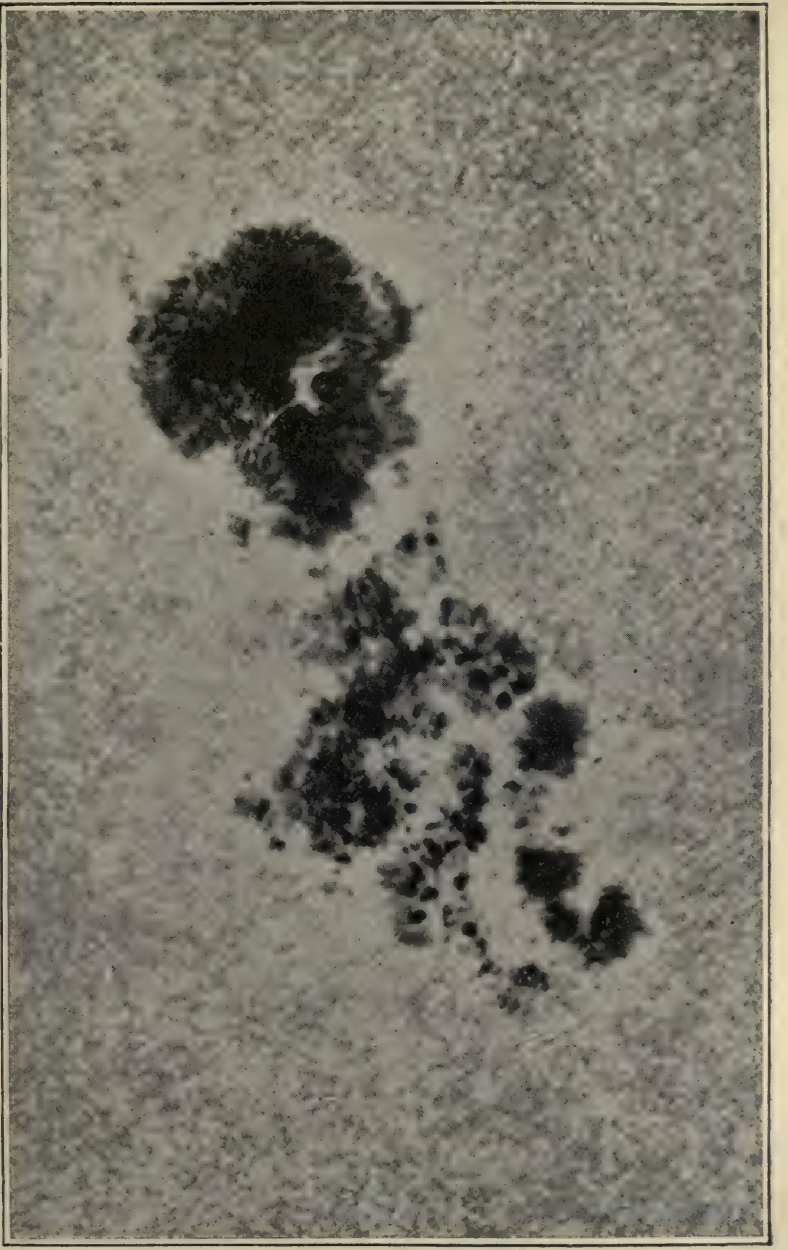


FIG. 28.—JANSSEN PHOTOGRAPH OF SOLAR SURFACE.

that therefore the interior of the Sun is so obscure as we should conclude from the interpretation of this photograph by the principles which apply to ordinary terrestrial pictures. The probability is that even the blackest part of that spot is in reality more brilliant by far than any illumination that we could possibly produce on the Earth by electric lights or in any other way.

FIG. 29.—SUN-SPOT. (*Langley.*)

No doubt the blackness which the photograph shows in the interior of any considerable spot is merely an effect of contrast; it would be impossible to expose the plate long enough to catch the light coming from the dark region without altogether over-exposing it so far as the luminous photosphere is concerned. We have no direct knowledge as to what the actual interior of the Sun may be like, but we shall be able to learn something by theoretorical considerations, of which more will be said in a later chapter. As we look at the picture, it would seem that we can actually trace on it the simultaneous effects of the two complementary modes of action. There is first the descent of the cooled vapours through the spot, and there is also the ascent of the highly-heated vapours which are

FIG. 30.—NASMYTH'S DRAWING OF A SUN-SPOT, SHOWING THE WILLOW-LEAF STRUCTURE.

to take their place; the blurred portions of the plate seeming to indicate by their rapid movements that they are the localities where supplies of heated vapours are tumultuously rushing up from below to take the place of the cool material descending.

In Fig. 28 we copy a photograph, also taken by Dr. Janssen, of the photosphere in a somewhat different mood. The same telescope, of about five inches aperture, was employed, with a wet collodion plate, to obtain it, and there is much interest in contrasting it with that last looked at. In the first place, the appearance of the photosphere is widely different in the two cases. The granular structure, so beautiful before, is now wanting; it is replaced by a surface in which the features are much less recognisable. Here, again, it will be noted that the black interior of the spot is separated from the general bright surface of the Sun by a wide penumbral margin. A striking illustration is also afforded in this picture of a phenomenon frequently presented by spots of this particular type. A bridge of luminous material, perhaps even brighter than the general surface of the Sun, stretches across the dark abyss in the interior of the spot. Here, too, are further indications of what seems a want of definition in the plate generally. This is no defect arising from any failure in the photographic process, or any accidental circumstances. It is certainly indicative of something really going on in the Sun. That this is so has been demonstrated in the most conclusive manner by Janssen, who has taken pictures in rapid succession of the same parts of the Sun's surface. Of course, if these "smudges"—for so they have been graphically termed—had arisen from any instrumental cause they would not be reproduced in the second picture, in the same localities, and under the same circumstances as in the first. This is, however, what we actually find to be the case. It is, therefore, certain that such marks on the photograph do not indicate blemishes incidental to the process; they must be interpreted as arising from certain changes in progress in the Sun. The only explanation that need be given is that to which we have already referred—namely, that the solar materials are in such rapid movement that they shift appreciably even during the limited time of exposure. The surface of the Sun is now studied with such assiduity that continuous photographic records of its appearance are obtainable.

Occasionally a great Sun-spot breaks out large enough to be visible to the unaided eye; and as a recent example we may mention that of February, 1892, of which an excellent description has been given by Mr. E. W. Maunder, assistant superintendent of the Solar and Spectroscopic Department at the Royal Observatory, Greenwich, who has specially studied it. During a part of the month just named, when the Sun was dull enough to admit of being looked at with the unaided eye, the great spot was a conspicuous object. The adjoining view (Fig. 31) shows a photograph of it taken at Greenwich on the 13th of February, 1892. In this case the scale employed is much less than in the pictures of Janssen's Sun-spots, for the entire disc of the Sun is now represented. It will be seen from the photograph that the spot consisted of two main portions

FIG. 31.—A PHOTOGRAPH OF THE SUN, FEBRUARY 13TH, 1892.

surrounded by a large number of small dark outliers; the penumbra is a very marked feature, and as it extends around the two central portions, it seems to include several of the smaller openings. We may also note that other Sun-spots besides the great group are represented on this picture. Thus, for instance, near the centre indicated by the intersection of two cross-lines, there is a small dark mark with two or three outliers, and on the left-hand side, near the Sun's limb, two or three Sun-spots can be distinctly discerned.

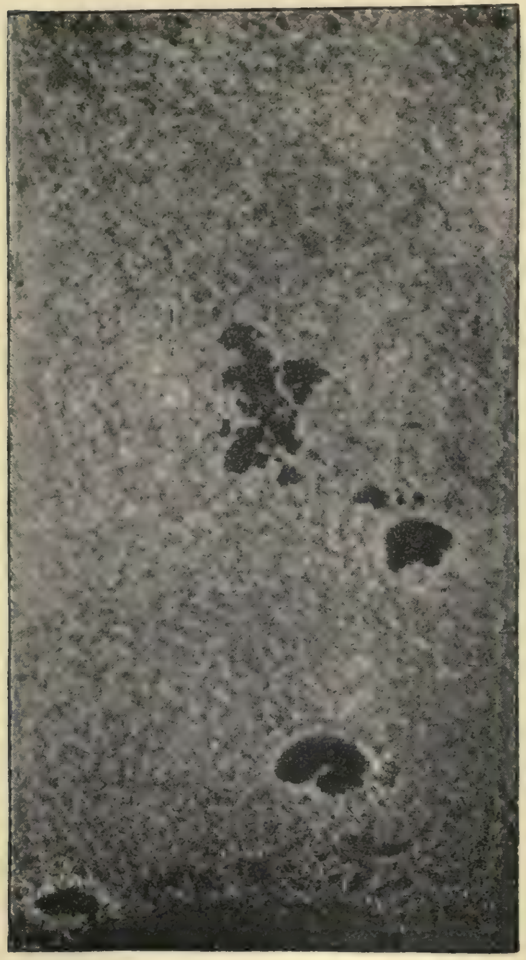


FIG. 32.—SUN-SPOTS AND FACULÆ.

This photograph shows the variations of solar brightness at different parts of the Sun's disc. There is a wide contrast between the maximum brightness at the centre of this picture, and the much feebler lustre at the margin. This is to be attributed to the vast outer atmosphere surrounding the Sun, which the rays of light have to traverse before they reach the Earth; in their progress they must necessarily suffer loss by absorption, and thus be deprived of some of their brightness. But rays which emanate from the margin of the Sun's disc have, as will easily be seen, to traverse an exceptional thickness of this cool atmosphere; they consequently undergo a special impoverishment, the effect of which is to produce a general darkening of the Sun's margin.

In Figs. 36 and 37 we reproduce two of the original drawings of Galileo, made at the time of his memorable discovery of Sun-spots, which were among the first fruits of his then recent invention of the telescope. We also give a sketch of a Sun-spot seen on the 31st of January, 1893, which was visible to the unaided eye (Fig. 40). This drawing, by M. Moreux, is a striking picture of the remarkable object which attracted so much attention at the time. The second view of the same feature on the 5th of February exhibits the alterations in its aspect as it approached the Sun's limb (Fig. 41).

I am also glad to be able to give a beautiful series of original drawings of Sun-spots by that accomplished observer, the Rev. Frederick Howlett (Figs. 34, 35). The explanation of them appears in his own words in a letter dated April 12th, 1893:—

"You will see that they are illustrative of the development of two different and remarkable groups. The first is the elegant *vorticose* group of May 11th, 13th, and 14th, 1863. The other is the uniquely characterised and *historic* group (may we not style it?) of August and September, 1859; when that notable outburst of drifting light—far exceeding the brilliancy of the solar photosphere—was observed by the late Messrs. Carrington and Hodgson about noon on September 1st of that year, and at which period perfect magnetic terrestrial *storms* were raging, and auroral displays, of extraordinary brilliancy, pervaded the greater part of the globe, both in the northern as well as the southern hemisphere.

"My drawings of August and September, 1859 (as well as those, too, of May, 1863), are, so far as I know, the only record on any large and detailed scale of those very interesting and very peculiar groups. Those of August and September, 1859, are preserved, though on a very small scale, in Mr. Carrington's classical work, *Observations of Solar Spots*, as well as in the series of Sun-pictures at Kew (photographic), though very much faded and originally *unequally* developed, and, again, on a very small scale.

"Observe, please, the *bifurcating* as well as *vorticose* arrangement of the outlying small spots and small patches of penumbra of the group lettered ξ, of May 11th, 1863, and the extraordinary manner in which it had developed by May 13th; wherein, too, observe the elegant way in which certain small and evidently connected penumbral specks *meander* amidst the group for a distance of at least 4′ of arc, or some 110,000 miles, evincing a peculiar far-extending drift in the photosphere, not surely without its significance."

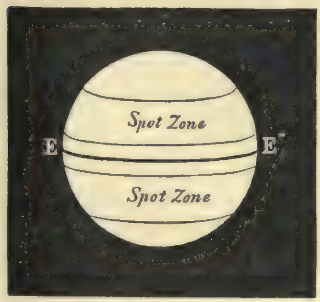


FIG. 33.—ZONES ON THE SUN'S SURFACE IN WHICH SPOTS APPEAR.

The distribution of spots over the Sun's surface is not uniform, and, indeed, they may be said to be generally confined within certain regions. This peculiarity is illustrated by the adjoining figure (Fig. 33), which defines two zones on the solar globe in which spots are particularly abundant. It will thus be noted that Sun-spots are never to be observed in the vicinity of the Sun's poles, and the equator seems also to be free from them. It must not, however, be inferred that Sun-spots are invariably confined within the limits herein indicated; for occasionally, especially in seasons of exceptional solar activity, these limits are considerably exceeded. Sometimes, too, it would appear that one hemisphere is much more marked than the other.

Professor Spoerer has collected a great deal of information bearing on the distribution of Sun-spots. It appears that in 1882 both hemispheres were about equally marked by these features, but the northern had many more spots than the southern in the years immediately preceding, while for very nearly a quarter of a century previously to that epoch they had been about equally distributed between the north and the south. Professor Spoerer has, however, shown that in at least two still

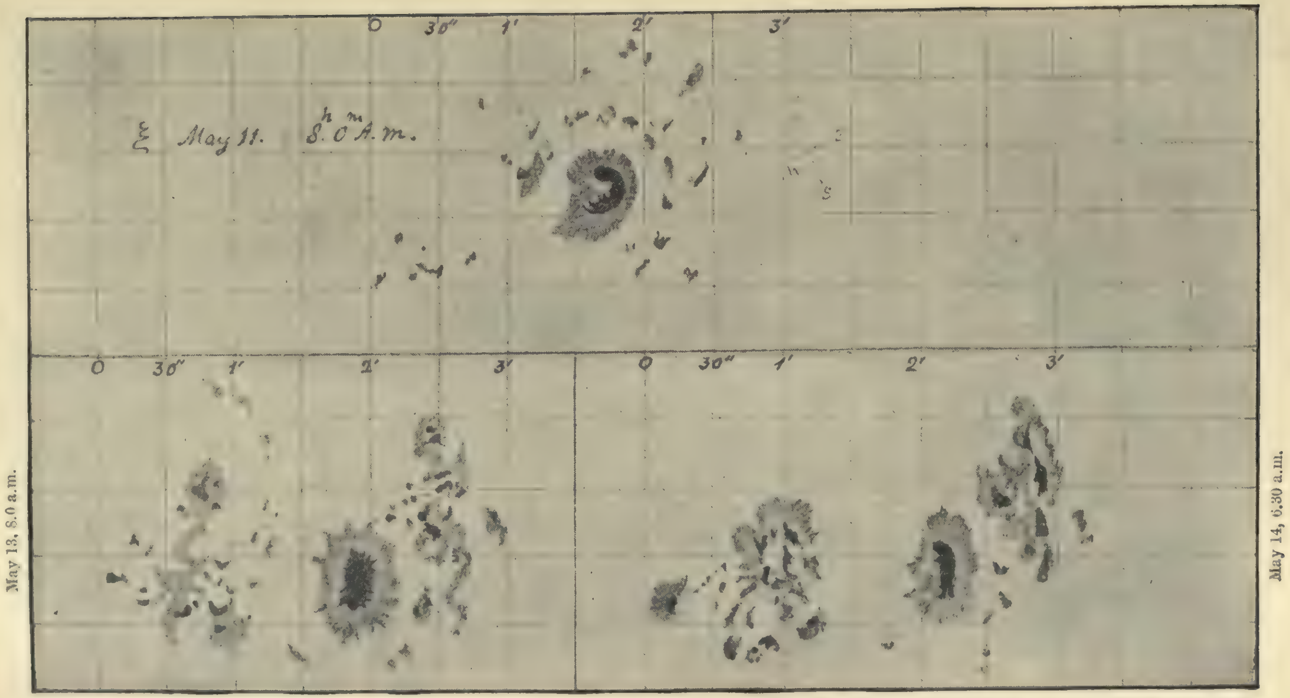


FIG. 34.—A BEAUTIFUL AND RARE VORTICOSE, DICHOTOMISING AND RAPIDLY CHANGING GROUP, MAY, 1863.

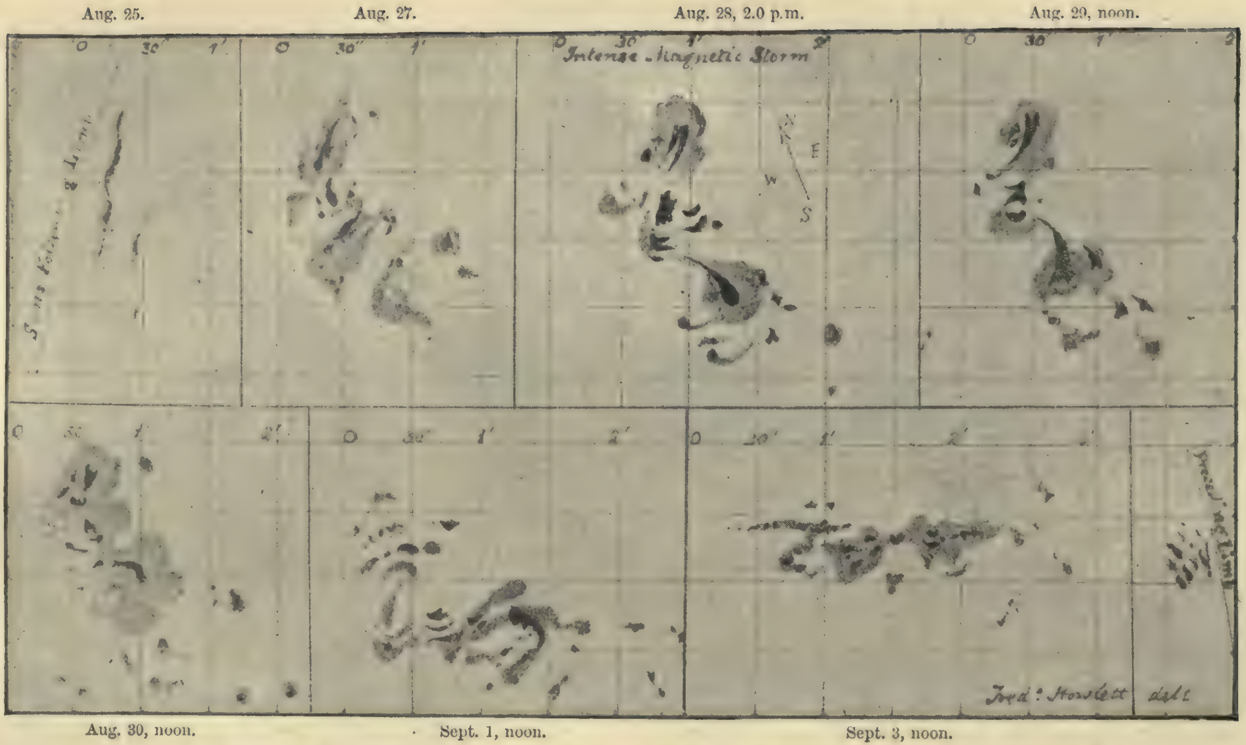
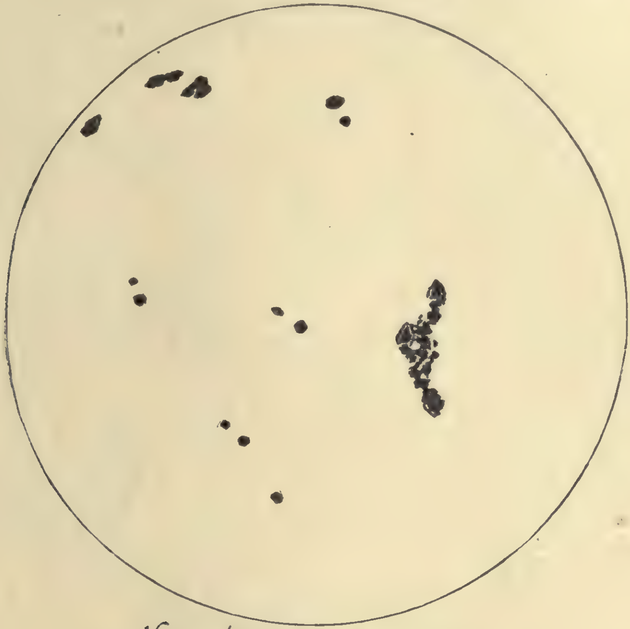


FIG. 35.—INTENSE MAGNETIC STORM, 1859. CARRINGTON'S AND HODGSON'S OUTBURST.

earlier periods the southern hemisphere was much more amply decorated than the northern. The first of these periods was from 1621 to 1625, during which the careful records of Scheiner show that there were periods in which there were no spots at all in the northern hemisphere, while the southern was well supplied. The second period corresponds with a remarkable phase in solar history. From the year 1672 to 1704 absolutely no northern spots were recorded; so that when a few broke out in that hemisphere in 1705, the two astronomers Cassini and Maraldi described it as a remarkable circumstance, for they had never seen spots there before.



1612. Dir. 3. May

FIG. 36.—GALILEO'S DRAWING OF SUN-SPOTS (1).

The appearance of three northern spots in 1714 was, at the time, referred to in the French Academy of Sciences as a remarkable interruption to the law which had seemed for forty years previously to confine the spots to the southern hemisphere of the luminary. The period just referred to seems, in fact, to have been a remarkably quiescent one as regards solar phenomena. No doubt the observers in those days were very few and far between when compared with the multitude of astronomers who now take interest in such matters. We do not, however, rely on merely negative testimony in making this statement. Distinguished astronomers have specifically mentioned circumstances which show that the Sun-spot activity must in those decades have been at a very low ebb. For example, Flamsteed, after mentioning, in 1684, that he had just observed a spot, said that it was the first he had seen for eight years. In 1705 it was stated that for sixty years two groups of spots had hardly ever been seen on the Sun simultaneously. This dearth of solar phenomena seems, however, to have terminated in 1716, in the autumn of which year no

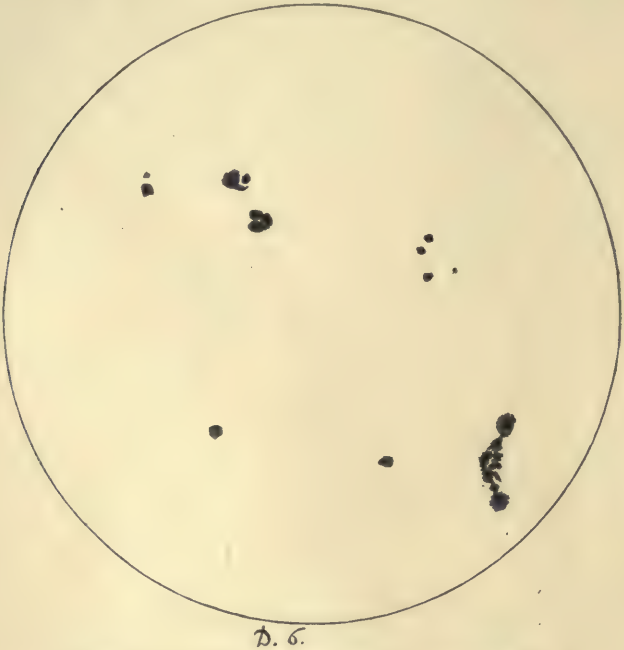


FIG. 37.—GALILEO'S DRAWING OF SUN-SPOTS (2).

fewer than eight groups of Sun-spots were visible on the Sun at the same time.

The instruction which we derive from the study of Sun-spots is of a very varied character, but perhaps their most obvious use

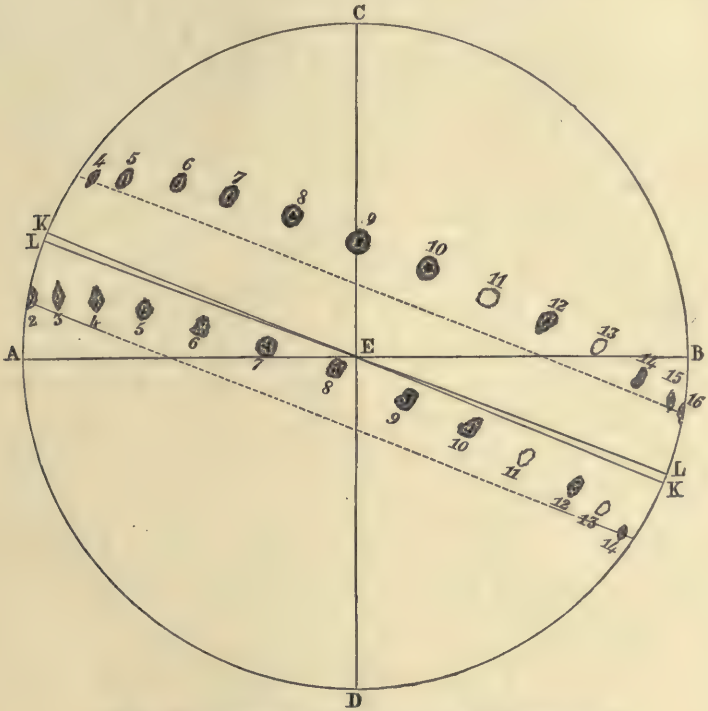


Fig. 38.—Scheiner's Observations on Sun-spots.

is to determine the rotation of the Sun on its axis with some approach to accuracy. So far as we are aware, every one of the celestial bodies rotates upon its axis; indeed, it would be in the highest degree unlikely, or I might rather say infinitely improbable, that any globe poised freely in space should be devoid of rotation. The Sun forms no exception to this general principle, and the spots give us the means of determining the circumstances of its revolution. It is, of course, obvious that the spots are not perfectly adapted for the purpose of measuring accurately such a phenomenon as rotation. We have seen that

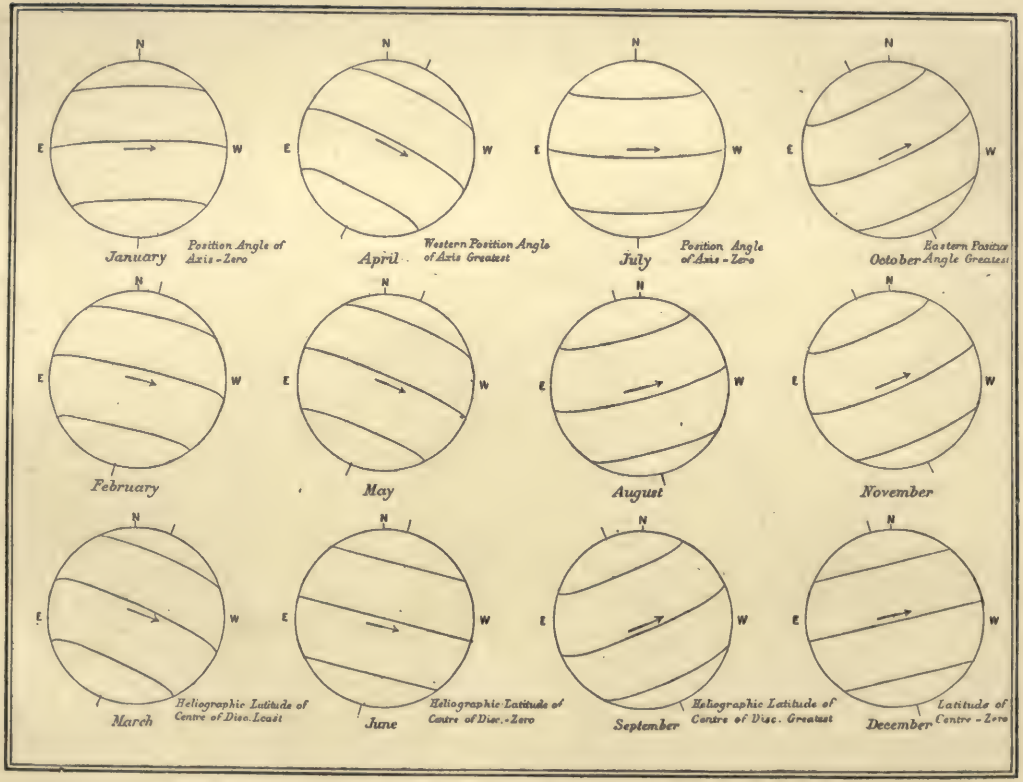


FIG. 39.—THE PATHS OF SUN-SPOTS AT DIFFERENT SEASONS.

these solar features are very transient in their nature; and doubtless, also, they frequently travel about on the surface of the Sun. But many of the spots endure long enough to be carefully followed during one, or two, or even more complete revolutions. The spot generally begins by appearing at the limb of the Sun, and then advances across the surface. Day after day it can be watched in its progress, until, after the lapse of a fortnight, it has traversed the surface from limb to limb; then it goes round behind the Sun, to re-appear again at the edge from which it started, the total interval being about twenty-five days. It may, indeed, happen that the spot vanishes altogether before it has had time to complete one rotation.

As hundreds of spots have been studied for the purpose, it is quite possible to eliminate from the final result any uncertainty which might arise from the individual peculiarities of each particular object. We are thus enabled to determine the duration of the Sun's rotation with the requisite accuracy. When several investigations were thus completed of spots on different portions of the solar surface, a very unexpected result became manifest. If the Sun were a solid body, then the duration of its rotation derived from one spot should be precisely equal to the duration indicated by a spot at any other latitude, northern or southern. This is perfectly obvious, for a similar statement with regard to the Earth would merely amount to saying that the period of the Earth's rotation must be the same for a point in England as for a point on the Equator.

The most striking result of Mr. Carrington's researches upon the period of the Sun's rotation, as determined from Sun-spots, was to demonstrate that the length of that period depended on the latitude of the spot employed in the determination. Let us give illustrations of some of the results at which Mr. Carrington arrived. It appeared that for a spot on the Equator the period of solar rotation was twenty-four days, two hours, eleven minutes; at twenty degrees of north solar latitude the rotation period seemed to have increased to twenty-five days, seventeen hours, and eight minutes; while at fifty degrees north—which was the highest latitude to which his observations extended—the period was no less than twenty-seven days, ten hours, and forty-one minutes. A noticeable diversity in the apparent periods was remarked in the other hemisphere. At a southern latitude of twenty degrees the period of rotation was twenty-five days, seventeen hours, and fifty-two minutes; while at forty-five degrees south latitude—which appears to have been the zone nearest to the south pole to which Carrington's observations reached—the rotation period had lengthened to twenty-eight days, eleven hours.

The observed discrepancy between the angular velocities of the Sun's rotation, as determined from different points on its surface, admits of no other explanation than the very simple one that the Sun does not revolve in the same manner as a solid body must necessarily do. And what is specially remarkable is that the equatorial regions are those which advance most rapidly. In fact, it seems as if the photosphere moved round the Sun in such a way that the parts near the Equator hurried on faster than they would do if the Sun were a rigid body.

It is just possible that there may be a movement in our own atmosphere which to a certain extent illustrates the solar phenomenon to which we at present refer. When the dust from Krakatoa was hurled twenty miles upwards into the air in 1883, it revealed the existence at those altitudes of an equatorial current of air circling round the Earth from W. to E. in thirteen days. Suppose that some outside observer had been measuring the velocity of the Earth on its axis, and did so by noting some fixed object—say a mountain range—then, of course, the period of rotation would have appeared to be twenty-three hours and fifty-six minutes, which we know to be its correct value. If, however, a cloud of Krakatoa dust whirling round in this lofty atmospheric current had been discernible by this supposed observer, and if he had taken its movements as an index to the rotational velocity of the Earth, he would have concluded the equatorial zone had a periodic time some two hours less than that indicated by the other observations. A somewhat similar phenomenon is, it may be remarked, presented in connection with the rotation of the planet Jupiter; for it appears that his equatorial region has a shorter periodic time than that derived from markings at a considerable north or south latitude.

So important is this question of rotation in its relation to the constitution of the Sun, that it is desirable to bring to bear upon it all the different lines of research we can. There are other methods of investigating the rotation of the Sun besides those provided by Sun-spots. Near the Sun's limb brighter patches than the photosphere generally presents are often seen. Such patches, usually termed faculæ (Fig. 32), often lie in the vicinity of remarkable spots. Faculæ, it is true, are inconstant in their characters, and are not very clearly defined, so that in some ways they are not well fitted to be the means of ascertaining a precise geometrical constant like that of the Sun's rotation. But it is so important to examine this question fully, that they have been pressed into the service for the purpose of testing the evidence afforded by spots.

A valuable contribution on this subject has recently been made by Dr. J. Wilsing, of the Astrophysical Observatory at Potsdam. The investigation was based upon solar photographs obtained at Potsdam in 1884; on these pictures 144 groups of well-marked faculæ could be identified so as to be available for the purpose of the required measurements. Dr. Wilsing distributed these faculæ into zones, according to their latitudes, from twenty-four degrees in the northern hemisphere to thirty-three degrees in the southern. It is certainly remarkable that, so far as these observations go, they seem to point to a revolution of the faculæ in equal periodic times, as if all attached to a rigid shell. This is certainly quite an unexpected result; and if it could be regarded as thoroughly established, it would necessarily lead to a grave reconsideration of much that was previously accepted with regard to the rotation of our luminary. There are, however, grounds for thinking that such determinations by faculæ are open to serious objections. The faculæ themselves are often ill-defined, and it is certain that they were sometimes not to be discerned on the photographs at times when they ought to have been visible if Dr. Wilsing's identification of them had been quite satisfactory. We must, therefore, consider that the result at which he has arrived requires further confirmation. In the meantime it is interesting to note that the length of the solar period to which he has been conducted is twenty-five days, five hours, twenty-eight minutes. This corresponds to the rotation period for spots at the latitude of ten degrees, as given by Carrington's observations.

There is yet another way in which the question of the rotation of the Sun admits of being studied. Here we employ the spectroscope, which, having already thrown much light on the character of the materials present in the photosphere, has now to be invoked with the totally different object of affording some information as to the conditions under which the rotation of the Sun on its axis is conducted. This necessitates, indeed, a very refined investigation, and it will be requisite to enter with some little detail into the explanation of the process employed.

We have already had occasion to point out how the position of a line in the spectrum depends on the wave-length to which that line corresponds. Suppose that the source of light is approaching directly towards the observer, then the number of undulations he receives in a second is proportionately increased. We can, indeed, submit the matter to actual numerical calculation. I shall suppose that the observer is receiving light of which the wave-length is the one-thirty-thousandth of an inch. Suppose that the object at which he is looking happens to be approaching at the rate of twenty miles a second; then, during the lapse of one second, not only all the vibrations emitted from that body in the course of a second will beat upon the observer, but in addition as many undulations as there are in the interval of twenty miles, through which the body has advanced during the second. On the other hand, if the body were retreating, the waves to which it would give rise would reach him with diminished frequency; in fact, if the body retreated rapidly enough, no light at all would reach the eye.

An illustration may perhaps make the matter more clear by employing the language of the corpuscular theory of light. It used formerly to be maintained that light consisted of excessively minute particles which were projected from the luminous body, and thence, after a journey from the source to the observer, entered into the eye, and gave the impression of vision. Of course, this theory has been displaced in modern days by the undulatory doctrine as to the nature of light; but still there are certain phenomena of which the corpuscular theory can be made to render a simple account, and it certainly illustrates the matter at present before us in a very convenient manner. If a rifle be fired towards a target, then, under ordinary circumstances, the velocity with which the bullet strikes the target will depend simply on the velocity with which the powder discharged the projectile, due allowance being made for the resistance of the air. If, however, the rifle itself were, by some device, to be moved away from the target at the moment of discharge, then the actual pace with which the bullet would speed on its journey must be less than that due to the explosion of the gunpowder. The difference would correspond to the rate at which the rifle was being drawn backwards. Suppose, for instance, that the speed at which the bullet was leaving the rifle was a thousand feet a second; if the rifle itself were mounted on a carriage by which it was flying backwards at a hundred feet a second, the pace with which the bullet would start could not be more than nine hundred feet a second.

If, however, the carriage on which the rifle were placed happened to be advancing towards the target at a speed of one hundred feet a second, then the initial velocity with which the bullet would fly forwards would be one thousand one hundred feet per second. If the rifle had an advance movement at the rate of five hundred feet a second, the pace at which the bullet set out would be one thousand five hundred feet a second. Neglecting now the effects both of gravity and atmospheric resistance, let us suppose that there were some means of determining the velocity with which the bullet struck the target; we should then have the means of learning whether the rifle were moving towards the target or away from it. This may illustrate the principle of the spectroscopic method of determining the movements of bodies along the line of sight. If the body be coming towards the observer, the velocity with which the luminous corpuscles arrive will be greater than if the body were stationary; if it be receding from the observer, then the velocity with which the corpuscles are animated will be less than if the body were stationary. The colour of the light on the corpuscular theory depends on the pace with which the particles arrive at the eye. Any movement, therefore, of the source of light along the direction of the line of vision will correspond to a variation in the hue which that light exhibits to the eye.

A method of measuring such variation is provided by the lines in the spectrum; hence the measurement of the shift in position of these lines enables us to determine whether a body is moving towards the Earth or is moving from the Earth. This process is not only one of extreme beauty, from a theoretical point of view, but it admits of being applied with a high degree of precision. In fact, Sir W. Huggins, our pioneer in astronomical spectroscopy, has declared that in all probability the function of the spectroscope in revealing the movements of bodies along the line of sight may ultimately prove more important than all the other astronomical applications of the same instrument. It will, in particular, be noted that the displacement of a body along the line of sight is just that element of motion which in the case of the stars the ordinary means of observation entirely fails to declare. In the particular application, however, with which we are at present concerned—viz., the examination of the solar period of rotation—the spectroscope is merely to be regarded as supplementing those other modes of determining the same quantity to which we have already referred.

It appears that as the Sun is rotating, the eastern limb of the luminary must be approaching, while the opposite one is receding from the observer. If, therefore, the light admitted to the spectroscope be taken exclusively from one limb of the Sun, the shift of the lines must show that the source is approaching; while if the light be taken from the other limb of the Sun, the shift of the lines must indicate that the source is receding. These anticipations can be verified in a striking manner by an instrumental arrangement which conducts the two spectra thus obtained into contiguity. It can then be easily seen how the two sets of lines are oppositely shifted, a ready method being thus afforded of determining the quantity sought for; for when the shift of the lines has been measured, the result can be translated into the velocity in miles per second with which that source is animated. Thus the spectroscope declares the number of miles per second with which the equatorial part of the Sun is approaching when viewed at one limb, or receding when viewed at the other. As we know in this way the number of miles per second with which a point on the solar equator is moving, and as we also know the number of miles in the circumference of the Sun, it follows that we have the means of determining from spectroscopic researches alone the period which the Sun requires for each rotation on its axis. It will be noted that in the application of this method we are dependent neither on spots nor on faculæ; but only on the photosphere itself.

One of the most complete investigations of this description has been recently made by Mr. Henry Crew; he employed as the dispersing apparatus of his spectroscope a Rowland grating with 14,436 lines to the inch. From 445 distinct observations he concluded that the equatorial period is about 26·23 days. It should, however, be mentioned that Mr. Crew's observations have failed to afford any very satisfactory information as to the differences in the rotation-period belonging to various solar latitudes.

There are thus wide differences in the equatorial period as obtained by different processes. Hornstein, from a discussion of the daily range in the barometer, has fixed the period of the solar rotation at 24·12 days. Braun and Hornstein, from the study of magnetic terrestrial phenomena, assigned in different investigations the values 24·18 and 24·51 days. Spoerer, from Sun-spots, arrived at 25·23; Wilsing, from observations of the faculæ, makes the length of the period also 25·23; while Mr. Crew, from the spectroscopic method, makes the result one day longer. Mr. Crew has suggested that a possible cause of these discrepancies may be found in the possible origin in different solar strata of the various phenomena observed.

A valuable series of observations of Sun-spots has been made by Dr. C. Braun, at Kalocsa, in Hungary. Dr. Braun assiduously observed the great luminary through fifty of its consecutive rotations; and the results at which he has arrived are of considerable interest, though no doubt we shall in the future attach greater weight to information derived from solar photographs than to visual observations such as those made by Dr. Braun.

His instrument was a refractor of four inches aperture, and the apparatus employed for the purpose of facilitating the work of the draughtsman is a very ingenious one, which might perhaps be made more extensively useful in astronomical research of this kind. The principle of the contrivance is easily stated. The light is reflected from the eye-end of the telescope through a right-angled prism, which produces an image of the Sun in a convenient position on the drawing-board. By this device Dr. Braun made 5,000 drawings of spots between the years 1880 and 1884. The chief aim of this observer's labour was to accumulate materials illustrative of the way in which spots are formed, and of the relations which spotted areas of the Sun bear to solar prominences.

His elaborate researches assist us towards tracing the natural history, so to speak, of each of the more remarkable spots. We learn the epoch when a famous spot broke out and the epoch when it disappeared, as well as certain other circumstances connected with its development, maturity, and decay. From these plates it appears that there were about sixty-one spots which admitted of being identified after having apparently completed at least one entire circuit of the luminary. We naturally look to these for information with regard to the period of rotation of the Sun on its axis. The observations at Kalocsa, though exhibiting in some places rather discordant results, still confirm, on the whole, our belief in the symmetry of the two solar hemispheres—in so far, at least, as the velocities of spots at different parallels are concerned. The observations also disclose what, indeed, other astronomers had likewise noticed, as to the idiosyncrasies of certain spots in regard to their velocities. Even on the same parallel some spots will be found which move more or less rapidly than others. But while the actual numerical determinations of the Kalocsa observations leave us in uncertainty as to some points, there are interesting results of a more general kind which they may be held to establish or to confirm. It has long been noted that when a period of abundant Sun-spots was commencing, the objects first began to appear in comparatively high latitudes. Then, as the season of solar activity wore on, the spots drew nearer and nearer to the equator before they finally ceased to exist, as that particular cycle of solar disturbance drew to a close. Under ordinary circumstances it has been observed that when Sun-spots are at

FIG. 40.—SUN-SPOT, JAN 31ST, 1893 (*Moreux*).

their maximum the latitude of the zones in which they occur is about sixteen or eighteen degrees. These details, however, admit of considerable variation.

From these different considerations it is perfectly obvious that there is a wide contrast between the actual circumstances presented in the rotation of the Sun and those which would be found if the Sun revolved like a solid body. If we suppose the Sun to be a fluid mass, it becomes possible—to some extent, at least—to trace out on mathematical principles the laws according to which the movements of its various portions should be conducted. Among the investigations which have been directed to

FIG. 41.—SUN-SPOT, FEB. 5TH, 1893 (*Moreux*).

the elucidation of this question we may mention those of Belopolsky, now of the Pulkowa Observatory. Let us suppose a liquid globe, the interior parts of which perform their revolutions in a longer time than the outer regions; then it can be shown, on the assumption of a reasonable law with reference to the way in which the velocities of the several shells vary from the surface to the centre, that currents on the outer surface of the globe would set out from both poles towards the Equator. On the other hand, if the core of the globe revolved in a shorter time than the exterior portions, then the currents of the fluid on the surface would set in the opposite direction—namely, from the Equator to the poles. The theoretical expectation that such should be the result has been experimentally verified.

This research has a specially instructive bearing on our attempt to interpret the course of events in the interior of the Sun. We want to learn whether the inner parts are revolving with more or less angular velocity than the exterior shells which are accessible to our observations. It would seem as if the movements of the spots in latitude ought to be capable of affording the necessary criterion. And on the whole, speaking quite generally, the movement of each individual spot may be regarded as tending towards the pole, in so far as any change in latitude is at all perceptible. This has been made clear by the researches of Spoerer and Carrington. As this shift in the place of the spots is due to a surface flow, it follows apparently from the principles laid down that the interior parts of the Sun must be rotating more rapidly than the exterior parts. It is, however, well known that the zones of spots, as the season of activity advances, approach more and more towards the Equator. This statement, however, does not really conflict with what we have said above. It merely affirms that the solar area in which spots are generated gradually approaches the Equator; but it does not invalidate the assertion that when any individual spot has been produced its drift is generally towards the pole. These investigations seem to hold out considerable hope that mathematical research may ultimately lead to a knowledge of the physical state of the Solar interior.

As yet, however, it is difficult to assert anything with regard to the actual nature of the movements in the interior parts of the Sun. It is supposed by Belopolsky that the density of the solar globe gradually increases as its centre is approached, according to the same law which is generally assumed in the case of the Earth. On this supposition gravity would attain a maximum at a depth of about one-sixth of the radius below the solar surface. If the shell of solar material which occupied this position were revolving at the maximum velocity, then the outlying layers would be carried onward by friction. It can then be shown that the effect would be to produce a velocity of rotation of the Sun as determined at different latitudes precisely in accordance with the law which Sun-spot observations reveal. According to this view, the most rapidly rotating internal solar shell would accomplish its revolution in 21·3 days, the other parts of the Sun revolving with an angular velocity less than that thus indicated.

As to the average velocity of rotation we cannot speak with precision ; nor, indeed, can we readily attach any very definite signification to such an expression as the mean angular velocity of the Sun. We may, however, in the following manner define what is implied. Suppose that at any moment all the materials of which the Sun is constituted were conceived to become rigidly connected in their precise actual positions, and that there was no longer to be any shifting of one shell of solar material relatively to another; the whole mass would then rotate as one piece, and the actual velocity with which it would commence to move may be taken to mean the Sun's average speed of rotation. Observation does not inform us as to what this average speed may be. It must certainly be more than we might infer from the study of the movements of the superficial layers, while it must certainly be less than the dynamical theory indicates.

In all probability the magnetic phenomena connected with the Sun will ultimately be our safest guide to the solution of this question. It will be shown in a subsequent chapter that the phenomena of solar spots appear to be intimately connected with those of terrestrial magnetism, which may thus serve to indicate the period of rotation of the Sun. For though it cannot be said that we have any very clear idea as to the precise nature of the physical relation between solar activity and terrestrial magnetism, yet it seems almost impossible to doubt that magnetic phenomena, in so far as they stand in relation to the Sun, are connected with it as a whole, and do not merely depend on the superficial layers open to our observation. Thus, the important constant which expresses the Sun's mean angular velocity may probably be given with greater accuracy from the observations of terrestrial magnetism than by any other method. Hornstein in this way determined the period at 24·5 days. So far as our present knowledge of the subject extends, these figures may be taken to represent the mean period of the Sun's rotation.

Our knowledge of Sun-spots in recent years is largely due to the extraordinary assiduity with which Signor Tacchini has studied them at the Collegio Romano. Admirable accounts of these investigations are given from time to time in the very useful astronomical notes in the columns of *Nature.* I shall here present an abstract of the results at which Tacchini has arrived; and in order to give definiteness to the epoch we are to consider, I shall commence with the year 1884, when a maximum display of spots was recorded. Under ordinary circumstances, the interval between two successive Sun-spot maxima is about 11·1 years; but that this period is liable to very considerable fluctuation is obvious from the fact that the maximum of 1870 was not succeeded by another maximum in 1881, as should apparently have been the case if the usual law had been obeyed. It was not, indeed, until February, 1884, that the Sun-spots had again reached a maximum. Tacchini investigated the different indications of solar activity as revealed by spots, faculæ, and prominences, and in all these different forms a maximum of activity was well marked. In 1885 the Sun was in a state of continued agitation in the neighbourhood of its equator. In 1886 there was a great falling off in the numbers of Sun-spots; for while in 1884 the luminary, when observed at Rome, was not found to be free from one or more spots on any day of that year, yet a stage of almost total quiescence lasted from October 31st to December 12th, 1886, so that during an entire rotation and a half the Sun was practically free from spots—indeed, for forty-two days only one tiny spot was seen, and that visible on no more than six days. It has been noticed that a similar state of temporary lull has been observed on previous occasions to succeed an epoch of exceptional activity. The decline in Sun-spots continued during 1887, the mean daily spotted area being only two-fifths of what it was in 1886. With the approach of 1888 it became obvious that the minimum must be very near at hand. The reduced number of spots made it plain that the least active part of the eleven-year period had been now reached, and there were several considerable intervals during which no spots at all could be seen. It was worthy of notice that the faculæ had not at the same time declined to anything like the extent noticed in the spots. The prominences were also in 1888 reduced in number, and attained considerably smaller altitudes than the displays of similar phenomena in preceding years. M. Brougier made a series of observations in 1889 with the special object of determining the exact date of the Sun-spot minimum. It appears that from April 11th to June 15th—that is, for an interval of sixty-six days—the Sun was completely devoid of spots, except certain very small ones, which appeared on two or three occasions in May. The minimum, therefore, appears to have occurred about the end of April.

From our observations of the movements of the spots, we have been able, not only to discover that the Sun has a rotation on its axis, but also to determine the position which that axis occupies. The latter problem would be quite a simple one if the axis happened to be exactly perpendicular to the Earth's orbit, and if the solar equator coincided with the ecliptic. These conditions are not, however, satisfied, and consequently the situation of the Sun's axis bears a relation to the surface of the sphere which changes with the seasons, and accordingly demands special study. I would recommend the student who is anxious to get definite ideas on a rather troublesome subject to make a little model for himself in the following way:—

Suppose the Earth's orbit is a circle, for its eccentricity is too small to require attention in this particular subject. Suppose, then, that a circle be drawn and a series of positions marked around it which the Earth occupies on several days in the year.

Take on this circle the point corresponding to the Earth's place on the 5th of June, and draw the diameter of the circle passing through this spot. Take now a small globe on which the poles and the equator and a number of parallel circles have been inscribed. Place this globe in a hole cut through the centre of the orbit, so that the plane of the globe's equator shall just intersect the plane of the orbit along the diameter just drawn; arrange the inclination of the globe's axis so that the angle between that axis and the perpendicular to the plane of the orbit shall be seven degrees. Take care also that the north pole in the upper part of the globe shall be tilted over towards that part of the orbit which the Earth traverses in September. The little globe then occupies exactly the same position as that of the Sun with regard to the orbit of the Earth.

The circles of latitude drawn on this globe indicate how the spots appear to move with the rotation of the Sun. From the study of this little model it will be easy to understand the diagram given in Fig. 39, which depicts the paths of spots across the Sun's disc. These pictures represent the Sun, not as shown through the inverting telescope, but as seen directly in the sky. The spots, of course, are first noted at the eastern limb of the Sun's disc, unless the point where they first break out should happen to be turned towards the Earth at the time the eruption takes place. In January the north pole of the Sun at noon coincides with the uppermost point of the Sun; the path of an equatorial spot would be, as marked in the diagram, from east to west along a track departing but little from the equator. On the 6th December the Earth happens to be in the line of nodes, and consequently in the plane of the Earth's equator. At this time, therefore, the paths followed by the spots across the Sun's disc are projected into straight lines. Again on the 5th June the Earth has reached the opposite point of its orbit; it will then be again in the plane of the Sun's equator, and, as shown in the diagram, the paths at this time pursued by the spots will also appear straight lines. On the 4th of March, however, the Earth, being then at its greatest distance from the nodes, will be depressed to the greatest possible extent below the Sun's equator, and consequently the paths of the spots will appear as elliptical as is possible for them, with the convexities of the ellipses turned towards the North. On September 6th the system is viewed from the opposite side of the orbit, and the curves reappear, only now they are convex towards the south. It will be noticed that on June 5th and on December 6th, both the north pole and the south pole of the Sun are just visible on the edges of its disc. From June to December, the north pole only is visible, and from December to June, the south pole only is visible.

It is necessary to bear these matters in mind when observing, for it is otherwise impossible to locate the spots in the true positions which they actually occupy on the solar surface. Astronomers have adopted a conventional way of indicating the position of the Sun's axis. Think of the Sun at noon, and then imagine the meridian drawn from the centre of the Sun up through the zenith to the pole. The actual north pole of the Sun will be somewhere in the vicinity of the upper point of the Sun's limb where it is intersected by this meridian. It will, of course, in general not lie exactly on the edge of the hemisphere turned towards us, but that is not material for our present purpose. The angle between this pole projected on the Sun's disc and the north point is what we call the position angle of the Sun's axis, and it is reckoned as positive if it lies towards the left, that is, to the east. If the Sun's axis lie to the right, that is, towards the west, then the position angle is regarded as negative. On July 6th the position angle of the Sun's axis is zero. Then it gradually increases day after day until, on October 10th, it reaches its greatest value of 26° 20′ in the positive direction towards the east. The position angle then begins to diminish and gradually declines until it becomes zero on January 5th. After this the axis crosses towards the west; its position angle is then measured negatively, and it increases until it attains its greatest negative value of 26° 20′ on the 5th April. Then again the decline begins, and the zero value is reached once more on July 6th.

The observer who desires to obtain accurate positions of Sun-spots will need to use diagrams founded on the principles we have just explained, in order to indicate exactly the part of the Sun on which the objects at which he is looking are situated. So far as the latitude of the spot under observation is concerned there need be but little difficulty in conceiving how it is to be measured. Let us suppose that the image of the Sun is projected on the screen in the usual way. It will in the first place be necessary to determine the cardinal points of the picture. Let us suppose that the observation is made at noon, the upper part of the screen being marked N, the lower part S, the part to the right E, and the part to the left W; just, in fact, as in an ordinary terrestrial atlas. It is supposed, of course, that the observer, while looking at the picture which his telescope has produced on the screen, has his back towards the Sun. Knowing the day on which the observation is made, he gets at once the position angle of the Sun's axis. A line should therefore be drawn on the screen making this angle with the vertical; this line will represent the direction of the axis of the Sun and the perpendicular thereto will be the projection of the Equator. But this, of course, will not suffice to enable the latitude and the longitude of the spot to be immediately determined. For, inasmuch as it does not generally happen that the axis of the Sun actually intersects the Sun's limb, the parallels of latitude and longitude will be found projected into elliptic figures.

The determination of the precise whereabouts of Sun-spots is rather a technical matter, and much greater space than we can devote to it here would be required to go into the matter fully. I may therefore refer to my "Atlas of Astronomy," in which I have given the ingenious method devised by Mr. Arthur Thomson. The observer will there see how, by holding the diagram in the proper position for the day on which his observations are made, he will be able to read off at once the actual latitude of the Sun-spot. In the determination of the longitude of the spot questions arise of a very different character, and here we encounter a serious inconvenience, from the fact that the Sun is not a solid body. We measure our terrestrial longitudes from Greenwich, and what we want is some stable mark on the Sun to serve as an invariable point from which to measure longitudes on the Sun, just as we measure terrestrial longitudes from Greenwich. There is of course no fixed point on the great luminary which can be so adopted. We are obliged then to assume a certain arbitrary point, and to suppose that this goes round the Sun once every 25·38 days, and think of it as the origin from which we can measure the longitude. The method is sufficiently accurate to enable us often to identify the spots noted by one observer with those noted by another.

It seems impossible to doubt that systematic fluctuations in the intensity of solar activity do actually take place. In default of any sufficient explanation on physical grounds for the occurrence of such periodic phenomena, we are led to look for any analogies which may suggest a clue to their interpretation. As we have already pointed out, it is clear that instability must arise in the Sun from the incessant interchange between the cooling vapours at the surface and the highly heated parts from the interior, which rise up to take their place. That such interchange should take place, not in an uniform manner, but by spasmodic periodic outbursts, is quite consonant with our experience in the boiling of liquids, and in other cases where large heated masses are involved. It is impossible in this connection not to be reminded of the geysers, and of the ingenious explanation given by Tyndall of their periodicity. Indeed, he demonstrated by the construction of an artificial geyser that his explanation was the correct one. If the thermal phenomena of the geyser, depending, as they do, on the passage of heat from the inside to the outside, take place, not gradually but by a violent outbreak at intervals which recur with tolerable regularity, is it not likely that a somewhat similar periodicity should be manifested in the Sun? It is true that the interval between two outbreaks of a geyser is measured only by hours, or by days, while the interval between two periods of exuberant solar activity is measured by a decade of years. But this discrepancy is not more than might be expected from a consideration of the mighty scale on which the phenomena of solar activity are exhibited.

CHAPTER IX.

SOLAR PROMINENCES.

WHEN a picture of the Sun is produced on a screen by a telescope, its edge, or limb, as astronomers prefer to call it, is a clearly defined circle; so, too, when a photograph is taken by exposing a plate to the Sun, the image of the luminary which is obtained presents a circular disc with a definite outline. When we ascertain the diameter of the Sun and state that it measures thirty minutes, we refer, it is always understood, to the apparent diameter of the disc which the Sun exhibits in either of the ways referred to. We have now to learn that there are large and important appendages belonging to the Sun, which extend far beyond the limit defined by the limb, either as we see it with our eyes on the sky, or as it is produced by the telescope on the screen, or as we find it depicted on the photographic plate. In fact, the visual disc presented to our examination by all these different methods is defined by the outer limit of what astronomers call the photosphere. No doubt the greater part of the Sun's mass is contained within this sphere, but it would be highly incorrect to suppose that the volume of the Sun extended no further than the sphere's boundary. We shall presently see that the true volume of the Sun, or the extent of space which its surroundings occupy, is immensely greater than that of the sphere which we ordinarily think of when we refer to our luminary. The surroundings of the Sun belong as veritably to it as does our atmosphere to our globe.

It is true that under ordinary circumstances we are not able to see these outlying portions of the Sun except by very special and indirect methods; but none the less do they belong to the Sun, the only difference being, that whereas the photosphere impresses a brilliant image on the eye, the solar regions which lie exterior thereto only emit, notwithstanding their vast extent, a light so faint that when contrasted with the splendour of the more brilliant part, it fails to excite our nerves of vision. We may here recall the well-known circumstance that in broad daylight there are many stars overhead which can be seen with the telescope when the sky is clear, notwithstanding that they are not visible to the unaided eye. Such stars are nevertheless at the proper season brilliantly visible at night. The fact is, that when the retina is suffused by the glorious light of the Sun, the

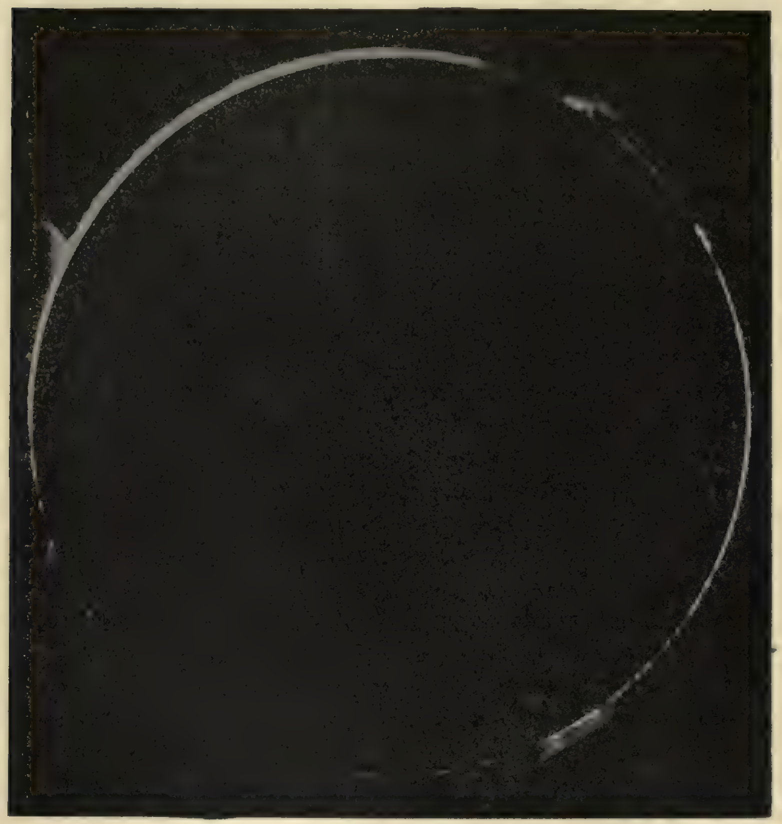


FIG. 42.—THE PROMINENCES AND THE CHROMOSPHERE AS SHOWN BY THE SPECTRO-HELIOGRAPH (*Hale*).

nerves are rendered insensible to light so feeble as that which even the brightest star can send us. A telescope for this purpose assists our vision in a twofold manner; in the first place, it intercepts a great deal of the diffused sunlight which would otherwise obtain admission to the eye and thus dull its acuteness; and in the second place, the telescope collects and concentrates the light from the star into a single point on the retina. From this illustration it will be evident that it would be quite illogical to infer that solar objects cannot be present in the regions exterior to the photosphere, merely because they cannot be seen under ordinary circumstances. Nor would it have been difficult to assure ourselves by a well-known fact of observation that the apparent limits of the bulk of the Sun, as they are defined by the outer surface of the photosphere, cannot really indicate the boundary within which the materials strictly belonging to the Sun are confined. We have already pointed out that the visible brightness of the Sun's disc declines as the observer looks from the centre of the luminary towards its limb. This fact is only intelligible on the supposition that there must be a copious exterior atmosphere enveloping the Sun and extending far beyond the margin indicated by the photosphere. Here we have at once a demonstration that the brilliant margin of the visible solar disc cannot be the boundary of what we define as the Sun.

Considerations suggested by the molecular theory of gases also lead us to the belief that the rarer parts of solar materials extend to a prodigious distance from the luminous centre. We know that gas is composed of molecules animated by extremely rapid movements; we know too that in these molecules an exceptionally high velocity is the concomitant of an exceptionally high molecular temperature. It is therefore certain that the stupendous velocity that must accompany temperatures so elevated as those which are characteristic of the Sun, implies a wide diffusion of solar materials. For, in the outer regions of our luminary, it will frequently happen that the molecules, darting forth with the exceptionally rapid pace which they occasionally attain, would ascend from the solar surface to a prodigious elevation before the gravitation of the central mass had succeeded in overcoming the outward velocity of the molecule, and make it commence its return. It might thus happen that a molecule would not merely ascend many hundreds or thousands of miles above the solar surface; it might attain a height as great as that of the Sun's radius or diameter, or even go much further still. What can happen to one molecule can befall millions of molecules, and hence it might have been confidently anticipated that there would be a wide diffusion of the solar materials around the central brilliant sphere.

Until within the last quarter of a century there was only one method by which we could become visually acquainted with the solar surroundings. To render them perceptible it was essential that the excessive light from the central globe should be intercepted. There is only one way by which this is possible if the phenomena are to be witnessed by the unaided eye, or even by mere telescopic observation. We are indeed indebted to a noteworthy circumstance for the opportunity of witnessing these solar phenomena which are ordinarily invisible. We have already described the conditions under which an eclipse of the Sun takes place, and noticed the remarkable coincidence that the apparent diameter of the Moon is on an average almost exactly the same as the apparent diameter of the Sun. At all events the Moon, by this fortuitous circumstance, serves completely the purpose of enabling us to inspect the solar surroundings. When the Moon is placed centrally in front of the Sun it intercepts all the direct light from the luminary, and as the size of our satellite is sufficient, but not much more than sufficient, to cover the photosphere completely, we are permitted so long as the total eclipse lasts to see the surroundings of the Sun in the most perfect manner.

Of course, such occasions present great varieties in their suitability for the observation of solar phenomena. It is essential that the eclipse be total, because, if even the smallest part of the photosphere remain exposed, it will diffuse a light quite sufficiently bright to extinguish the feeble radiance of such objects as we want specially to observe. It is also to be noted that even when the eclipse is total at any part of the Earth, the presentation of such a phase is limited to a very narrow region, and can only last at any one situation for a very few minutes. Expeditions have generally to be made to distant countries by those who want to investigate these phenomena. The most noticeable feature on such an occasion is the pearly-coloured luminosity, called the corona, which extends to a vast distance round the Sun. At present, however, what we intend to discuss are the much smaller features, known as the solar prominences, lying close to the Sun's margin at the base of the corona. The corona itself will be considered in the next chapter.

It was in the year 1842 that prominences first received attention from the students of solar phenomena. It was not, however, until the eclipse of 1868 that the character of these objects, as really appertaining to the Sun, became properly understood. In these days it seems a little difficult to realise how there could have been any doubt about the matter. However, as the Moon was a necessary factor in the exhibition of prominences, inasmuch as at the period of which we speak there were no other known means of seeing them, it was not at all clear that our satellite might not be in some way concerned in their actual production. Even at the present time our knowledge of the true character of these prominences would be very imperfect, were it not that a beautiful method which does not require the assistance of a total eclipse has been fortunately devised for their observation.

The primary fact demonstrated by these observations testifies that the Sun is completely surrounded by a more or less regular shell of coloured gaseous matter, lying immediately outside the photosphere. This outer layer has been called the chromosphere. It constitutes the base of that portion of the Sun's surroundings which is ordinarily invisible. The chromosphere is clearly liable to stupendous agitation, in consequence of the mighty thermal disturbances to which the Sun is subject. In the progress of these agitations tremendous volumes of these glowing ruddy gases are projected aloft from the chromosphere, and thus attract attention. But our knowledge of what we call prominences or ejections, whose existence was indeed primarily demonstrated by eclipses, was shortly afterwards greatly increased by the adaptation of that new method, whereby protuberances could be scrutinised without the necessity of waiting for such opportunities as were afforded by eclipses. The honour of having invented the process which has revolutionised our methods of studying solar phenomena is shared between two distinguished astronomers, Dr. Janssen and Sir Norman Lockyer. It is, however, right to add that the principle on which the method is based had been previously indicated by Sir W. Huggins, to whose spectroscopic labours astronomy owes so much. The principle of this process, which is now universally employed in the study of the solar atmosphere, is so important that it requires a somewhat detailed exposition.

In our chapter on the solar light we have explained the general principles of spectroscopic analysis; it has been there shown how the light from the photosphere generally forms what is called a continuous spectrum, produced by a combination of rays of all the hues of the rainbow. This band of richly-coloured light is, it is true, marked over by thousands of dark lines. It must therefore be understood that when physicists speak of it as "continuous," they use this language with the object of contrasting a spectrum of the ordinary solar type with a spectrum of bright lines, such as that which is generally yielded by an incandescent gas. An excellent instance of a bright-line spectrum is afforded by hydrogen when heated in a Geissler's tube by the passage of the electric spark. The gas in this case exhibits a spectrum consisting only of a few brilliant lines, separated from each other by long, dark intervals. This may be regarded as the type of what is known as a "discontinuous spectrum." In the observations of the eclipse of August 18th, 1868, the spectroscope was applied for the first time to the examination of the phenomena presented by the Sun's surroundings. Dr. Janssen discovered that, while the photosphere exhibited the continuous spectrum characteristic of ordinary Sun-light, the chromosphere, of which, as we have seen, the prominences are exuberant developments, displayed a discontinuous spectrum of bright lines. This was a very instructive observation. It demonstrated in a conclusive manner that the chromosphere was composed of vast masses of glowing gas, while the precision with which the positions of the bright lines contained in its spectrum could be measured gave rise to the expectation that the actual elements present in the chromosphere would admit of being determined. These expectations were realised; no fewer than nine lines were identified, by comparisons of their positions in the spectrum, with those occupied by the lines of terrestrial elements. It was thought at first that both sodium and hydrogen were present in the chromosphere. However, what was taken for the sodium-line is now known as the "helium-line." Janssen was so much struck with the brightness of the lines while the total eclipse was in progress, that he made an exclamation at the time, intimating that he expected to see the same lines afterwards without the aid of any eclipse. On the following day he was enabled to realise this expectation by an ingenious utilisation of the peculiar character of the light emitted by the material of the chromosphere. If the light from the prominences had been similar in quality to that of the photosphere, so that the only difference between the two arose from the relative faintness of the photospheric material, there would have been no possibility of applying the method which Janssen found so successful. But, as we have already stated, the spectrum of the chromosphere is composed of bright lines, while that of the photosphere is continuous, and this difference is all-important.

To illustrate the principle, let us suppose that a trace of faint red light, of only one particular refrangibility, is mixed with an intensely bright white light of the ordinary composite character. Under these circumstances it would be impossible to distinguish the red light, or even to be aware of its presence in the mixture. But if we could attenuate the white light sufficiently, while leaving the intensity of the red light undiminished, then we might hope that the red ray, even though intrinsically feeble, might be able to proclaim its existence. This is what the spectroscopic method of investigation enables us to accomplish. If the mixed light be passed through a prism, the white constituent is, of course, opened out into all the colours of the rainbow, and dispersed over the entire length of the spectrum; the red light, however, though deflected from its path by the action of the prism, is not opened out, for, as it is composed of rays each possessing the same degree of refrangibility, it is impossible for it to be weakened by dispersion, however much it may be bent. We thus see that the red light will preserve its brightness, while the white light will be transformed into a series of coloured portions faint in proportion to the dispersion used.

By passing the light through a second prism, the dispersion of those rays which were originally white will be carried further; they must submit to be spread over a spectrum of greater length, while the red line still continues compressed within its original limits, and consequently retains its original brightness. We can repeat the process so often that at last the light which was originally white has become attenuated into a long coloured band, the actual intensity of which, at each point, has been reduced below the brightness of that red line which has suffered no appreciable diminution. It is thus plain that we may now expect to see the red line on the background of which the lustre has been sufficiently reduced. Such is the principle of the method by which Janssen and Lockyer have shown us how to observe the solar chromosphere and the solar prominences without waiting for those rare opportunities which are afforded by total solar eclipses. In the actual application of the process, an image of the Sun is produced by the telescope; the slit of the spectroscope is then brought to the edge of the solar image along the direction of a radius of the disc. If there be a prominence at this point, some of the light which it emits, along with a considerable quantity of the light from the photosphere, will be admitted into the spectroscope. By the action of the prisms in the way already explained, the white light of the photosphere can be gradually attenuated, while the light of the prominence, being concentrated into lines, will have undergone but little diminution. Such lines will therefore be visible through the spectroscope, and thus the existence of the prominence will be demonstrated.

But the spectroscope is able to render us an even more emphatic service. It can be employed for the actual production of a distinct telescopic picture of the very shape and features of the prominence. Let us fix our attention for the moment on some one particular line in the light of the prominence—say, for example, the line C, which is known to belong to hydrogen. Suppose that the slit of the spectroscope be opened so widely as to allow the whole extent of the image of the prominence to pass through, then as its light consists solely of rays which possess the particular refrangibility of the C line, they will all be deflected by equal amounts through the system of prisms. It therefore follows that when they fall on the second telescope of the spectroscope they will be in the proper condition for being observed through that second telescope, just as if they had diverged directly from the prominence itself without having undergone the deflection which the prisms impose. We shall thus be able to see the prominence itself as a telescopic object through the second telescope of the spectroscope.

The views obtained of these marvellous appendages to the Sun are among the most interesting and striking spectacles that the telescope discloses to the astronomer. Under ordinary circumstances in the observation of astronomical phenomena, the movements of most celestial bodies seem so slow from our distance that they cannot be said to be perceptible while the astronomer is watching the objects. A planet cannot be seen to move as we look at it, and still less is the motion of a binary pair of stars perceptible until after a protracted series of observations. No doubt by nice observation of a comet it may be detected in the act of passing the stars by which it is surrounded. In the observation of an eclipse of one of Jupiter's satellites we can note the gradual decline of the light as the little body becomes extinguished. In the occultation of a star by the Moon we have a phenomenon which is absolutely instantaneous in its character; but I think it will be admitted that the spectacle presented by a great solar prominence when viewed with a powerful spectroscope far transcends in interest any phenomenon of celestial change in actual progress which can be elsewhere observed.

In the first Plate we have represented the chromosphere and the prominences as given by M. Trouvelot, the accomplished student of solar physics. It should, however, be observed that these objects are not always of a ruddy hue. The picture exhibits the extraordinary agitation by which the chromosphere is affected.

It is impossible to look at prominences without being struck with the resemblance which they bear to gigantic flames. In fact, these objects have many of the characteristics of flame, as we see it issuing from our furnaces. The prominences are, as we know, vast masses of heated gas, which leap upwards from the solar surface; they even can be seen to flicker like flames (Plate IX.), and fragments from their upper parts are occasionally seen to break away from the main eruptive mass, just as happens in the mightiest terrestrial conflagrations. But the movements of these stately solar prominences are performed with the apparent deliberation which might be expected from their gigantic proportions. From a tongue of flame a hundred thousand miles long we need not expect to see flickerings perceptible in seconds, as they would be were the flames only ten or twenty feet in height; but allow us to say a few minutes, and in that period gigantic wavings to and fro will have been frequently noticed. It often happens that while the astronomer actually keeps the same portion of the solar limb under view, he witnesses the subsidence and disappearance of a mighty prominence, or perhaps he may be fortunate enough to see the outbreak of a fresh one.

As an illustration of exceptionally striking phenomena, I refer to observations made by M. Trouvelot, who has given an interesting account of the remarkable solar protuberances observed at Meudon between June and November, 1892. Amongst the most violent eruptions which have been studied we may specially mention that of the 11th July, 1892, of which a figure is given adjoining (Fig. 43). Before entering on the description of this object, I should, however, mention that the spectroscope renders the astronomer another service besides that of allowing the outlines of the prominence to be distinctly figured. We are indebted to the same instrument for the power to perceive and even to appraise numerically the velocities of the movements of the parts of the prominence, in so far at least as those movements happen to be directed along the line of the observer's vision. We have already had occasion to explain the principles by which the indications of the spectroscope can be made to afford information with regard to the movements of the body from which the light comes, along the line from the observer's eye to the object. If the body should be approaching the observer, the lines characteristic of its spectrum are shifted towards the blue end; on the other hand, if the body is receding from the observer, then the lines are moved towards the red end. By measuring the amount of the displacement, the velocity of the corresponding movement along the line of sight can be

FIG. 43.—PROMINENCE OBSERVED BY TROUVELOT, 11TH JULY, 1892.

determined. In this way we are able to tell, by the shiftings of lines in the spectra of the prominences, the amount of their movements along the line of sight. The knowledge thus acquired is supplementary to that afforded by direct measurement, which can determine movement only on the surface of the heavens. In the case of a violent outbreak of prominences, not only are the visual movements remarkable, but the alterations of refrangibility in the light, the tokens of corresponding movements in the mighty incandescent vapours from which the light emanates, are of a most striking description.

Fig. 44.—Eruption of 5th July, 1892.

On the day we have already named, 11th July, 1892, M. Trouvelot saw at 12 hrs. 19 mins. mean time at Paris, at the position indicated by 151° on the limb, that a violent eruption of incandescent matter was in progress. He was specially observing through the hydrogen line c, which gave evidence of exceptionally rapid movements. In fact this line c, which usually presents distinctly marked edges, could hardly be recognised in the spectrum of the chromosphere, except as a sort of flame. There were so many different elementary bodies in the gases of which this prominence was composed, that quite a large number of brilliant lines corresponding to these varied elements could be discerned throughout the whole length of the spectrum. These lines were so numerous, and often so evanescent and spasmodic in their appearance, that their identification was impossible. Indeed, the rapidity of the movements of the chromosphere caused so much uncertainty in the places of the lines, that it would have been impossible to determine the elementary bodies present.

How rapidly the movements of a prominence of this description are sometimes executed may be inferred from certain measurements which M. Trouvelot was fortunate enough to procure. When his attention was first attracted to the phenomenon, the principal jet of the group had attained a height of 197,000 kilometres. In five minutes it had sprung up to 427,000 kilometres. Thus the actual velocity with which this flame ascended from the solar surface cannot have been less than 715 kilometres a second. Three minutes later the altitude had sunk to 351,000 kilometres, the decline taking place at a mean velocity of 669 metres a second. Three minutes later again—that is to say, six minutes after the maximum height had been reached—this astonishing phenomenon had totally vanished. An interesting feature connected with this eruption, extraordinary alike for its violence and for its brevity, was that it appeared at a region of the Sun not usually the seat of much disturbance; at least so far as the indications given by spots and faculæ are concerned. It took place at the considerable latitude of 58° 50′ south of the equator, ordinarily a comparatively quiescent region.

The eruption observed on the 5th of July, 1892, is shown in Fig. 44. The spectrum yielded by the prominences on this occasion contained indications of the ordinary lines of hydrogen, C, F, and H; it showed the well-known double lines of sodium, the line belonging to the mysterious element helium, the lines of magnesium, as well as several other lines. But among the interesting observations made by M. Trouvelot, in the

Observatory at Meudon, I think that of the 7th of October, at 11.21 A.M., is the most remarkable. It showed a wonderful structure, termed by M. Trouvelot a black protuberance. Similar phenomena have also been noticed by him on certain other days. He describes the spout of ejected material as inky black, the temperature of its constituents, whatever their nature, having been probably much below that possessed by the ordinary luminous protuberances. Finally, this accomplished astronomer sums up the results of many years' observations of these phenomena in the following conclusions:—Firstly, that the solar eruptions giving rise to the protuberances have, in general, their seat in regions of the Sun occupied by spots and by faculæ, which themselves owe their origin to the same causes of agitation as those which produce the protuberances. Secondly, that the solar eruptions, even when they are of the most violent type, have in general only a very brief duration, not ordinarily exceeding some few minutes. Thirdly, that solar eruptions are usually accomplished, not by a single great outbreak, but

FIG. 45.—BLACK PROTUBERANCES OF 7TH OCTOBER, 1892.

by alternations of effort and repose, pauses of quiescence appearing to be necessary, in order, as it were, that energy might be accumulated sufficient for another display.

Professor Riccò has demonstrated that there is a periodic fluctuation in the latitudes of solar prominences corresponding with the observed variations in latitude of solar spots. He founded his conclusions on a valuable series of investigations undertaken at Palermo, during a period of eleven years, in which no fewer than seven thousand six hundred and fifty-three prominences, all attaining the considerable elevation of thirty seconds, were observed. The observations thus accumulated illustrate, in a striking way, the changes which take place in the latitude both of prominences and of spots during the progress of a Sun-spot maximum.

A remarkable instance of a protuberance with enormous velocity, observed on June 17th, 1891, is given by M. Julius Fényi, director of the Haynald Observatory, Kalocsa, Hungary. It appears that a column of the incandescent gases, which measured 111 seconds from top to bottom, was projected almost vertically from the Sun in a single mass, and ascended to a height of 257 seconds. The speed was so great as to reach 45 seconds of elevation per second of time ; indeed, the velocity with which the materials were animated seems to have transcended anything that has ever been before observed in the movement of a celestial body. The component of the velocity exhibited in the actual ascent of the prominence amounted to no less than 485 kilometres a second. This does not, however, fully express the extraordinary velocity which characterised this object. It happened to be moving rapidly toward the observer : this was demonstrated in a striking manner by the fact that the image of the protuberance on the C line remained all the time outside the slit, the normal position of that line being, in fact, entirely dark. The velocity of approach towards the observer thus indicated ranged between 337 kilometres and 449 kilometres per second at the top of the protuberance, while the lower parts exhibited velocities of even greater amount. There is good reason for believing that both the vertical velocity outwards from the Sun and the velocity towards the observer existed simultaneously. This being so, we can compound the two velocities in accordance with a known principle in order to find the resultant motion. We thus obtain the astonishing fact that the actual velocity with which these materials darted from the Sun must have amounted to 600 kilometres, or 375 miles, per second. Indeed, we may feel confident that the true velocity was even greater than this. Only two components of the movements were well ascertained. The speed in the third direction, estimated at about 100 kilometres a second, would raise the total higher still. It may be here remarked that the velocity with which these substances were animated greatly exceeds the velocity which would be sufficient to explode and drive them away finally from the Sun. This is a very instructive point, especially in connection with the theory that comets are objects which have been ejected from the Sun.

M. Fényi thinks that for the explanation of the extraordinary vehemence with which these masses are expelled in defiance of gravitation from the body of the Sun, it is necessary to suppose the existence of some special repulsive agent, nor does his suggestion seem unlikely that this agent may be electrical action. The effect of charges of similar electricity on two bodies would be to drive them asunder, and certainly it seems as if some such potent repulsive agent would be necessary to account for so vast an outbreak as that which has been here described.

The connection between the prominences and the spots is of such great interest that it is fortunate we have some striking evidence on the subject. M. Trouvelot has described a very interesting case in which it would seem that the fall of a solar prominence into a spot had been witnessed. On the 6th of August, 1891, he was observing a large group of spots approaching the western limb of the Sun, towards which they were of course being carried by the Sun's rotation. The three spots which formed the group were in close proximity, and they exhibited some peculiarities which arrested the observer's attention. From the southern edge of the penumbra on the central spot, certain long brilliant filaments crossed this spot as a luminous bridge, then traversed the southern penumbra of the third spot, from which they seemed to bend down, sink, and be lost in the depth of the opening; as they crossed the central spot the luminous filaments were sufficiently separated to allow the umbra to be visible between them, but beyond that they seemed to coalesce into a compact bundle.

On the 9th August the same group of spots was again observed. It was by this time very close to the Sun's limb, nevertheless the filamentous bridge could still be discerned. The spectroscope was now brought to bear on the phenomenon, and it was noted that a few jets of prominences were to be seen on the Sun's margin in the immediate neighbourhood. On the 10th August the group had reached the limb of the Sun, and could no longer be observed. The spectroscope, however, indicated the position of the spots, and brilliant prominences were now seen from a point situated at the identical part of the limb which was known to be the position of the umbra of the central spot. These various jets were enclosed by a luminous arch, the elevation of which at the centre was 160 seconds, and it covered a range on the limb extending to twelve degrees. Its position corresponded precisely with that of the luminous bridge of filaments which had been already observed crossing the group of spots. That this arched prominence and the luminous bridge were the same is confirmed by the greater width which the structure displayed at the south than at the north, as well as by its form and filamentous structure. We have consequently the extremely interesting result that this prominent arch and the filamentous bundle which was seen crossing the spot were merely different views of the same object.

This observation is an important addition to our knowledge of the relation of spots to prominences. It suggests that the bright bridges so frequently seen crossing spots are in general only prominences, displayed this time not against the sky on the Sun's margin, but against the black interior of the spot. M. Trouvelot thinks that there are two quite different classes of spots; one of these are of a comparatively inert character, and cross the limb without any display of associated prominences. A spot of the other class, an example of which we have just been describing, is clearly the seat of violent eruptions by which jets of incandescent matter are projected to great elevations. It appears in the present instance that the prominence was produced by the activity of the central spot of the group of three; and that after ascending to a great elevation, it then bent over into an arch until its extremity dipped towards the opening of the spot of the group which appeared to be in a state of repose. M. Trouvelot infers from his various observations that some sort of attraction exists between certain prominences and spots in a state of repose. This view would certainly tend to confirm the belief often entertained that certain spots are, as it were, sinks by which cooler gases descend into the Sun's interior.

In the recent development of the study of prominences, notable aid has been rendered by the combined resources of spectroscopy and photography. I shall here mention one of the results which have been arrived at, which appears to be of special significance in relation to the distribution of hydrogen, not only in the solar chromosphere, but throughout the universe. It was discovered by Sir W. Huggins that in the ultra-violet portion of the spectrum exhibited by the white stars, there was a remarkable system of lines which were clearly shown to belong to hydrogen. The importance of this discovery was much increased when it was shown by Balmer that this system of lines, together with the visible lines of hydrogen, were all clearly harmonics, so to speak, of some fundamental note in which the molecule of hydrogen vibrated. It should indeed be remarked that that very sagacious physicist, Dr. G. Johnstone Stoney, had already pointed out many years previously, that the visual lines of hydrogen were linked by an arithmetical relation which clearly pointed to the existence of some such theoretical law as that demonstrated by Balmer's researches. The whole set of ultra-violet hydrogen-lines were photographed in the solar spectrum by Dr. Schaster, at Sohag, during the eclipse of 1882; while Prof. Hale photographed five of them June 23, 1891; and M. Deslandres recorded two about the same time.

We have now to describe a very important new departure by which our knowledge of prominences and of faculæ is being greatly extended. No doubt a vast step was made when Janssen and Lockyer taught us how to observe prominences by the spectroscope without the necessity of waiting for an eclipse. But we were still only dependent on eye observations. It seemed possible that some photographic method should be found which might render to this branch of astronomy a like service to that which it had already rendered in other directions. This has now been accomplished. The prominences have been photographed. It is impossible to overestimate the stimulus that has thus been given to solar research. In a few minutes photographic processes will produce a far more accurate picture of the chromosphere than eye observations could have produced in hours. Here, as elsewhere, the photographic eye sees what no human eye can see. I proceed to explain this recent development of solar physics.

When the twelve-inch equatorial refractor was erected at the Kenwood Observatory, Chicago, in 1891, the director—Professor George E. Hale, an accomplished young American astronomer—proposed to undertake the application of photographic methods to the systematic study of solar phenomena in the most complete manner possible. Any success which had hitherto been obtained in this direction was chiefly confined to the representation of solar spots. No doubt photographic representations of faculæ were occasionally obtained when they exhibited areas sufficiently near to the Sun's limb. It is, however, well known that towards the centre of the solar disc faculæ invariably became wholly indistinguishable alike to the unaided eye and to the ordinary photographic apparatus. There could, however, be no doubt that faculæ must be present at other positions on the solar disc besides those in which our ordinary methods revealed them. Here there was clearly an opening for possible photographic work to surmount the difficulty in the observation of faculæ.

It was also plain that there was room for an important extension of photographic methods by applying them to the representation of the prominences. Besides the researches thus indicated, it seemed quite possible that the radiations from faculæ, spots, and prominences might exhibit interesting features in those ultra-violet rays which, being invisible to the eye, could only be rendered manifest by the peculiar assistance which photography renders. Such a measure of success has attended Professor Hale's work that he is now able to obtain photographs in which all the prominences around the entire circumference of the Sun are made visible by a single exposure. Through this method of studying solar phenomena there is not only a considerable saving of time, but, what is far more important, a greatly increased accuracy in the delineation of the objects. Professor Hale is now able by his process to render faculæ clearly and abundantly discernible even on the brightest part of the Sun's disc. We have here another illustration of the fact, now so well known, that photography is no longer to be regarded merely as a means of saving the time of the astronomer. We now regard the photographic plate as an agent for disclosing features in the celestial bodies which must have remained utterly unknown had it not been for this special assistance.

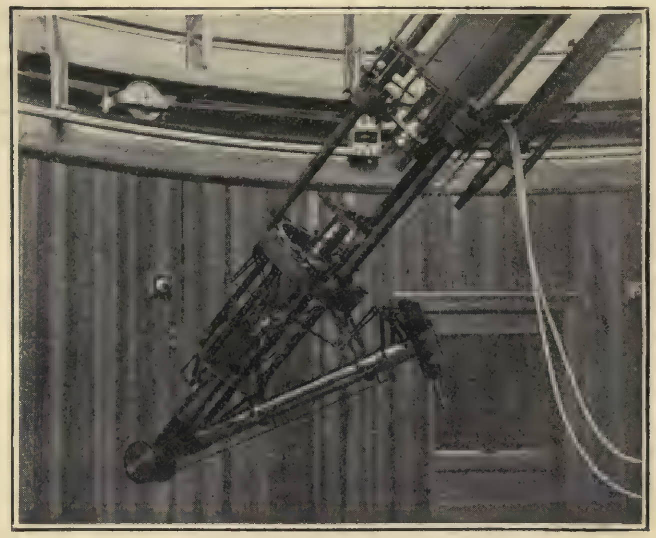


FIG. 46.—PROFESSOR HALE'S SPECTRO-HELIOGRAPH.

The apparatus that Professor Hale has invented he has termed the spectro-heliograph, Fig. 46. The essential principle of the contrivance may be said to consist in sifting out from each part of the Sun those particular rays which belong to the K line of the spectrum, and then photographing with the particular light thus obtained.

Let us suppose that the spectro-heliograph is employed in photographing the prominences round the margin of the Sun. The process employed is as follows:—A large equatorial telescope forms an image of the solar disc, about two inches in diameter. This image is crossed by a narrow slit, three inches and a half long. There is a practical advantage in having the slit adjusted parallel to the Sun's axis, because then, whatever marks are produced by such dust particles as generally happen to be present on the jaws of the slit, will be parallel to the solar equator, and thus serve to mark its position conveniently. The spectroscope then diffuses away, by its dispersion, the ordinary solar light, while it transmits, with almost undiminished lustre, the rays of definite refrangibility emitted by prominences.

Professor Hale has found for his purposes the light from that extreme violet portion of the spectrum, indicated by the letter K, to be the most suitable. Accordingly he sacrifices, not only all the white light of the photosphere, but also whatever light of refrangibility other than that of K may have come from the chromosphere, and survived the effects of attenuation by dispersion. To secure that no other light whatever, save that with the wave-length characteristic of K, shall fall on the photographic plate, the latter is covered by a screen, in which a slit is so placed that all the K light shall be permitted to pass through it to the plate, while all other light of every description is intercepted. The essence of Professor Hale's apparatus, in so far as procuring a picture of the whole solar disc is concerned, is a mechanical arrangement, whereby the slit in front of the

FIG. 47.—PHOTOGRAPH OF SOLAR PROMINENCE (MARCH 24TH, 1892).

photographic plate is moved in perfect sympathy with the shift of the other slit over the Sun's disc. The condition to be invariably fulfilled is that the light from the K line, and only that light, shall in each case be transmitted. We thus obtain actually a photograph of the Sun by K light alone. The singularity of this picture will appear to us all the more remarkable when it is remembered that the light by which it is produced is almost invisible. The requisite motion is imparted to this beautiful mechanism by a clepsydra, or water-clock, most ingeniously contrived by Professor Hale. The result is that in two minutes a photograph of the Sun can be obtained.

FIG. 48.—PHOTOGRAPH OF CHROMOSPHERE MADE WITH THE SPECTRO-HELIOGRAPH.

The benefits derived from such a method of promoting our knowledge of the luminary will be best appreciated by remembering that even the most skilled observer would require not less than an hour to make a complete survey of the solar margin, and to sketch the various prominences that will usually be met with. The special advantage in the photographic method will, however, be found in the study of those short-lived prominences characteristic of periods of special solar excitement. On such occasions photograph after photograph can be taken, and a complete and accurate pictorial history of the disturbance will be the result. This invention of the spectro-heliograph by Professor Hale is the most important contribution to our methods of studying the chromosphere since the time when Janssen and Lockyer first showed how the prominences could be observed without the assistance rendered by a total eclipse.

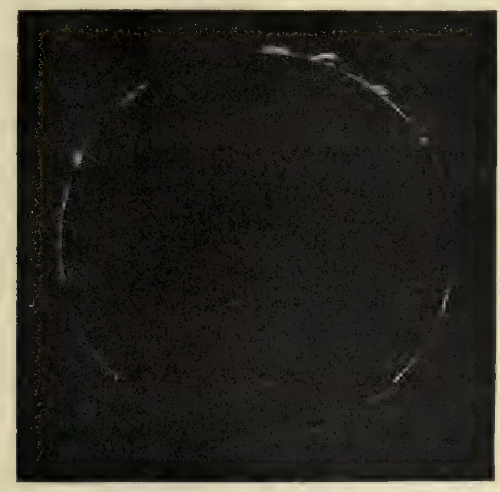


Fig. 49.—Chromosphere of August 8th, 1893. (*Hale.*)

To understand the method of obtaining photographs of the faculæ it is necessary to refer to the phenomenon known to spectroscopists as the reversal of the dark lines. Under ordinary circumstances, the solar spectrum is seen to be ruled with myriads of dark lines, but it occasionally happens that a line of exceptional brightness will be observed to occupy exactly the same position usually filled by some of the dark lines. Such an occurrence is said to be a reversal. There is no difficulty in accounting for the circumstances under which such a phenomenon as the reversal of a dark line may arise. Remember that the dark lines characteristic of the solar spectrum owe their origin to the gases surrounding the Sun, these gases possessing a lower temperature than the heated photosphere beneath. We know also that the lines produced by the selective absorption of photospheric light, as it passes through the cool exterior gases, occupy precisely the same positions as would have been filled by bright lines if the exterior gas, instead of acting relatively as an absorbent, had been sufficiently heated to have been itself a source of radiation. Ordinarily speaking, dark lines in the solar spectrum produced, let us say, by calcium, bespeak the presence of non-luminous calcium vapour in the Solar atmosphere. But when such lines are seen to be reversed, the phenomenon informs us that there must be abundant calcium vapour glowing with fervour in the region from which the light comes. The light which it radiates, possessing as it does the same particular refrangibilities as the dark lines characteristic of the same element, illumines, so to speak, the darkness, and crosses the dark intervals with bright lines.

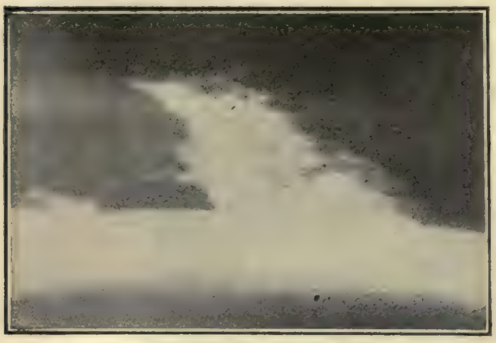


FIG. 50.—SUN PROMINENCE, OCTOBER 16TH, 1892. (*Hale.*)

Suppose, for instance, a prominence consisting of a vast mass of glowing calcium vapour is projected to such an elevation in the Sun's atmosphere that the brilliance which the incandescent vapour pours into the calcium lines more than compensates for the absorption due to the cooler calcium vapour outside, then we have what is called a reversal of those particular spectral lines. Sometimes the singular spectacle is presented which is known to spectroscopists as a double reversal; in this case the ordinary dark line is filled with

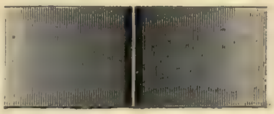
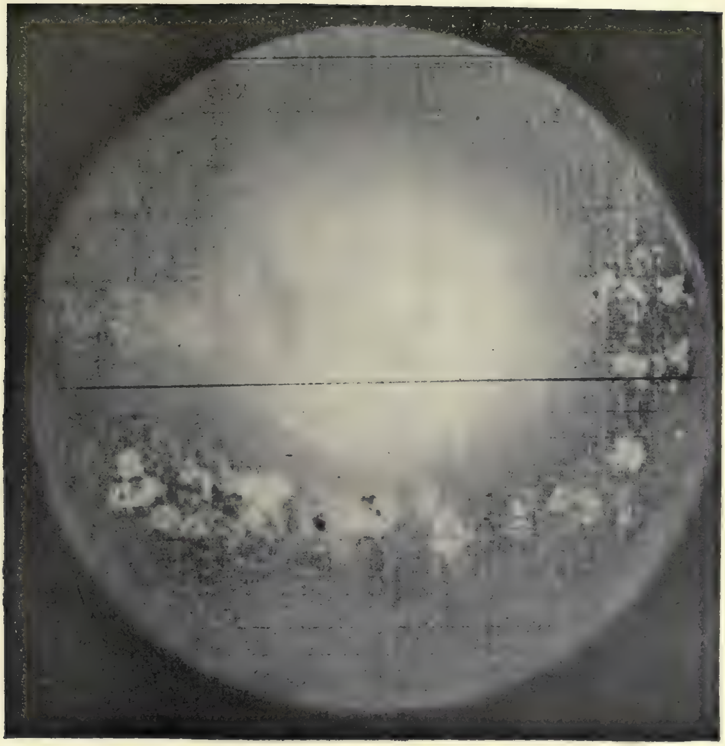
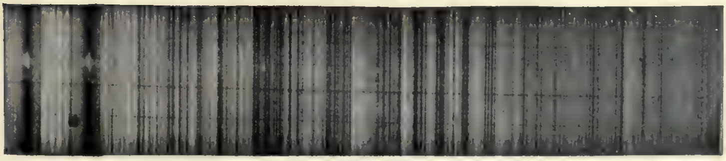


FIG. 51.—THE REVERSAL OF A LINE.

PLATE VII.



FACULÆ PHOTOGRAPHED BY SPECTRO-HELIOGRAPH (*Hale*).



REVERSAL OF LINES BY A PROMINENCE (*Hale*).

a rather broad band of luminosity, and down the centre of the bright band a fine dark line is found to be inserted. In this case we interpret the double reversal to be an indication of the presence of a volume of glowing gas overlaid by a not very copious atmosphere of the same gas in a cool state.

Professor Young, to whose researches on the chromosphere we are so much indebted, made, in 1872, an important remark with regard to the lines H and K. They were found, he said, to be regularly reversed in the penumbra, and in the immediate neighbourhood of every important solar spot. This interesting discovery showed that in the vicinity of Sun-spots on all parts of the disc faculæ are developed consisting of gases glowing with sufficient brilliance, not only to disperse the darkness found in certain lines of the spectra of those regions, but to replace that darkness by a vivid illumination. Professor Hale confirmed this discovery by taking photographs with the slit placed across the Sun in the neighbourhood of a large spot. He found that in six such photographs the K line was reversed in from three to ten places, most of these being doubly reversed by a dark line through the centre of the bright line. He has been enabled to utilise Professor Young's discovery in the happiest way to procure by the help of the spectro-heliograph photographs of the faculæ all over the solar disc. (Plate VII.; see also Fig. 66.)

Among the more striking solar prominences which have been witnessed in recent years, I may mention that observed by Professor J. Fényi at the Kalocsa Observatory on October 3rd, 1892. A sketch of this remarkable display of solar activity is reproduced in Fig. 52, and its importance may be judged of from the fact that the extent of the disturbed margin occupied no less than 30° along the Sun's limb — that is to say, one-twelfth of the entire circumference of the solar disc. The altitude which the prominence attained was 237,000 miles, which is more than half the Sun's radius, and the Earth is introduced into the figure to show its comparative size. The erupted gases were ejected with such violence that they assumed the form of a number of fragmentary portions, more or less isolated, some of which are described as being extremely bright. The numbers marked along the solar limb indicate the heliographic positions in accordance with the methods already explained for defining the various localities around the Sun's margin. In a couple of hours all the higher parts of this stupendous eruption had vanished. It is interesting to note that the remarkable prominence observed and drawn in Hungary was simultaneously photographed at the Kenwood Observatory in Chicago by the apparatus just described. Professor Hale remarks that, at the region where the prominence leaves the Sun to soar upwards, and again in the neighbourhood where the extremity of the

FIG. 52.—GIGANTIC PROMINENCE COMPARED WITH EARTH.

prominence dips down to approach the Sun's disc, groups of faculæ are exhibited on the photographic plates. It is impossible to look at a picture of this kind without having the eruptive character of the phenomenon strikingly suggested. It seems plain that the chromosphere is in an unstable condition, so that from time to time a terrific outbreak takes place, to be succeeded by a period of comparative quiescence.

To the same acute observer whom we have so often mentioned, Professor Hale, we owe an account of a remarkable solar outbreak which took place on July 15th, 1892. The locality of the Sun where this disturbance originated is one to which attention had already been directed. It was marked by a spot whose high latitude in the southern hemisphere and rapid changes of form had shown it to lie in a neighbourhood where changes of exceptional violence were in progress. I may here describe the results depicted on a series of photographs which were obtained at the Kenwood Observatory on the day named.

A picture taken at 11.8 a.m., Chicago mean time, showed no other remarkable symptom save that the surrounding faculæ seemed somewhat brighter than usual. Another plate exposed twelve minutes later demonstrated that a marked change had taken place, which we shall describe in Professor Hale's own words. "Extending between the umbræ, in a direction slightly inclined to the Sun's equator, was a perfectly straight and exceedingly brilliant object, which expanded slightly at its eastern extremity, and turned sharply towards the north, terminating abruptly in a brilliant ball just east of the centre of the northern umbra. The sudden formation of this remarkable object did not seem to affect the general group of faculæ surrounding the spot, for they remained in practically the same form as at first. As the plates were not developed immediately, we knew nothing of the disturbance, and the next photograph was not taken until about 11 hrs. 47 mins. Meanwhile an entire transformation had taken place in the luminous phenomenon, and so completely were the umbræ covered by the brilliant outbursts that they were no longer visible in the photograph. The straight tongue running between the umbræ in the first photograph had developed into an **S**-shaped form, similar in appearance to a facula shown in our photographs of the great February Sun-spot when on the eastern limb. Brilliant forms had also appeared to the north-west of the northern umbra, and the disturbance extended over an area of about four thousand million square miles."

No further exposures were made until 1 hr. 21 mins., by which time it would seem that the disturbance had practically ceased, and ere long the faculæ surrounding the spot resumed the same appearance that they had presented before the disturbance had commenced. Unfortunately but few visual observations of the outbreak were procured, for it was not until the disturbance was nearly over that Professor Hale was aware of the unique interest attaching to the photographs taken on that particular morning. At a quarter to one the C line was, however, visible in the spectrum of the spot, and the reversals were so brilliant that their forms could be well seen when the slit was opened sufficiently. Professor Hale remarks that as the faculæ surrounding the spot did not appear to have been materially affected by the disturbance, it would seem to follow that the phenomenon must have taken place at an elevation above the Sun's surface considerably greater than that of the faculæ. In connection with this observation, we are reminded of Mr. Carrington's celebrated observation in 1859, of a sudden vast outbreak, which left the details of the group of spots where it occurred sensibly unchanged, on the restoration of quiescence, from what they had been before the commotion began.

Among the most successful of Professor Hale's pictures taken with the spectro-heliograph may be mentioned that of the chromosphere and prominences obtained on May 21st, 1892, which we have represented in Fig. 42. The picture looks so like one of the photographs obtained during a total eclipse of the Sun that it is most interesting to note how it was actually secured. Professor Hale's photographs in the K line give, as we have seen, representations of the prominences; and for the purpose of the present picture the Moon's place was taken by a circular metallic diaphragm. It happened, however, not to be perfectly concentric with the solar image at the moment when the exposure was made. This explains why the depth of the chromosphere is not equal on opposite limbs of the Sun. In

Fig. 47 a direct photograph of a prominence obtained on March 24th, 1892, has been given.

Professor Hale's method is eminently fitted for the study of faculæ. In the older processes, where we were dependent on visual observations alone, faculæ received comparatively small attention. Now, however, that the method has been devised by which they can be seen and minutely studied all over the solar disc, their importance is greatly increased.

Faculæ are shown by this and other pictures to be far more extensive than the spots, both in number and in area; but we can here do no more than give the conclusions at which Professor Hale has arrived. He remarks, no doubt, that some of the opinions to which he has been led may, as the work develops, require certain modification.

Professor Hale records an interesting case in which a remarkable development of a solar prominence was observed by his assistants, Mr. W. B. Hale and Miss M. D. Hale, who were engaged in the regular work of the observatory during the absence of their brother, the Professor. It was on July 8th, 1891, at 23 hrs. 45 mins., Chicago mean time, that Professor Hale's observers noted the sudden formation of a prominence, which happened to be observed simultaneously at Kalocsa, the apparatus employed being a seven-inch refractor and a six-prism automatic spectroscope. It is interesting to note that the measurement of the altitude of the object, which at Chicago was put at 80,000 miles, is in good agreement with the 204 seconds which appeared to be the altitude at Kalocsa; this will be seen if it be remembered that a distance of 400 miles on the Sun subtends an angle of a second at the Earth.

In communicating these facts Professor Fényi takes occasion to mention that this great prominence is of particular interest, because in a neighbouring locality there happened to be at the same time a remarkable group of spots. It will be noted that there is always a difficulty in tracing the connection between protuberances and spots, inasmuch as for the protuberance to be visible, it must lie at the edge of the Sun, in which situation spots are not generally to be seen. Indeed, at the time when the great protuberance of which we are writing was observed, the group of spots with which it was associated had just passed round the edge of the Sun's limb and become invisible. Professor Fényi says that this particular solar locality has been evidently for some time past the site of remarkable solar disturbance. Just as in a terrestrial region like Krakatoa, where the heated materials of the deep interior find that some special channel affords them access to the surface, and permits the display of their powers, so the neighbourhood indicated by these solar observations is a place where the interior energies of the Sun find opportunities for causing great phenomena at its surface.

It would seem that the great prominence which happened to be simultaneously observed at Chicago and in Hungary, notwithstanding that it only lasted fifteen minutes, was the product of an eruption already noted as critical arising from this region. Indeed, the prominence was probably not far from the nearest great spot of the group, in which case the actual root of the mighty projection would have been round the limb on the remote side of the Sun at the time of the observation. Such, however, was the length of the great column that its upper portions were visible from this side, and formed the object which attracted so much attention. This view is rendered probable by the circumstance that no lines indicative of the presence of metallic vapours could be observed in the spectrum; such lines are usually developed only at the base of the prominence. In reading this I am reminded of the way in which I have seen icebergs in the Atlantic. It is unusual to be near enough to see the waves breaking at the base, for the base is actually below the horizon. It is, so far, in the same condition as the prominence which we have been considering.

In all photographs of the chromosphere, and of the prominences which are developments of it, H and K are the strongest lines represented. The photographs demonstrate that the material to which these lines owe their origin extends into the highest parts of prominences; it is, however, important to notice that no indication of the presence of this material has been found at any greater distance from the limb—that is to say, in the corona. Of these two lines it appears that K is always stronger than H, and extends farther from the limb. The prominences invariably exhibit the same forms when viewed through the medium of either of these lines. Certain minute differences that can be occasionally detected may be attributed to the greater brightness of the K line. Both H and K vary in width according to the altitude above the Sun's surface from which the light has been taken. They are broader at the base of the chromosphere than at its surface. Accordingly, photographs taken with the slit of the spectroscope tangential to the limb show these lines more than twice as broad as they appear in the higher region of the prominences. Both lines H and K are often seen doubly reversed in the chromosphere, in which case a narrow dark line runs down the centre of a broader bright line. In very bright prominences the whole series of ultra-violet hydrogen lines have been sometimes photographed. In the fainter prominences, however, the more refrangible members of this system are not to be discerned.

No prominence has been found which showed the H and K lines, without some of the hydrogen lines; and the form of any prominence observed in the line C is the same as in H or K, though, on account of the superiority of the latter lines for photographic purposes, more details are generally discernible. The spectra of eruptive prominences frequently contain many metallic lines in the ultra-violet. H and K are always reversed, and generally doubly reversed in the faculæ, which seem to surround each spot completely. Professor Hale thinks that the coincidence of H and K with the two strongest lines in the spectrum of incandescent calcium justifies the inference that these important lines are due to the element named.

As an illustration of the complex nature of the materials in a prominence it may be mentioned that the spectrum of one of these objects taken at the Kenwood Observatory on October 15th, 1892, showed no fewer than seventy-four bright lines in the ultra-violet part. This is the more remarkable when we reflect that not less than three object-glasses had to be traversed by this light in its passage through the instrument on its way to the photographic plate; and it is, of course, well known that ultra-violet light suffers especially in passing through even the most transparent glass. All the lines which Professor

Hale had previously photographed, as well as all those obtained by M. Deslandres with apparatus in which no glass was used, are shown on the photograph, besides thirty-two lines not previously known.

With this account of the prominences we conclude what has to be said about that shell of solar clouds known as the chromosphere, and of which prominences are merely developments projected outwards by great upheavals directed from below. But outside the region which they occupy extends a portion of the solar surroundings which is still of a mysterious nature. The corona, or faint pearly light, which is seen during a total solar eclipse, has a luminosity so delicate that it eludes all our attempts to observe it, except with such facilities as an eclipse affords. Considering the rarity of eclipses, it need hardly be deemed surprising that as yet we know but little with regard to the corona.

Originally we were also dependent on the opportunities afforded by eclipses for our knowledge of the prominences. So long as we had no other source of knowledge with regard to these objects, our acquaintance was but slight; indeed, it was hardly known whether they had any real existence at all, until a method was discovered by which we could dispense with eclipses. It may be hoped that when some method shall have been discovered by which the corona can be investigated without waiting for an eclipse, we shall learn much more about it than we know at present. As to what our knowledge is, let the following chapter tell.

CHAPTER X.

THE SOLAR CORONA.

ON the occasion of a total eclipse of the Sun, the margin of the luminary is found to be surrounded with a spreading extent of pearl-coloured light known as the corona. This is very variable in form. Sometimes the corona is comparatively uniform around the various parts of the margin of the solar disc. On other occasions vast projections are seen, extending at times to distances as much as twice the Sun's diameter. Indeed, in certain cases the projections are far longer still. Up to the present no efforts have been successful to render this marvellous solar appendage visible, except when such facilities are available as a total eclipse provides. This is doubtless one reason why comparatively little is as yet known with regard to the actual character of the corona, notwithstanding all the attention which has been devoted to the subject. It must be remembered that total eclipses, during which alone this solar appendage can be observed, are but few and far between; indeed it has been estimated that an astronomer who assiduously devoted himself for a period of thirty years to the observation of these phenomena could only secure about three-quarters of an hour of effective observation.

To obtain even this measure of work at the corona, it would be necessary for the astronomer to travel to any part of the globe where the eclipse might happen to be exhibited. The estimated time also supposes the observer invariably favoured by fine weather, an assumption which I need hardly say is not likely to be realised in practice. It should be noticed that even yet hardly more than a quarter of a century has elapsed since the corona was first definitely recognised as possessing a veritable existence as an addendum to the globe of the Sun. Before that time its very existence was often a matter of doubt; and even when the presence of the corona was indisputable, it seemed still possible that it might belong to the Moon rather than to the Sun. It might therefore be said that up to the present there has not been more than half an hour's really effectual observation devoted to the study of this notable solar feature. The best use has, however, been made of whatever opportunities have occurred. During recent eclipses, the precious minutes have been utilised to the utmost advantage. Not only have careful eye-observations been made of the outlines of the corona and of the brightness of its several parts, but it has been photographed with considerable success, while the spectroscope has been invoked for the purpose of discovering the materials of which this strange solar envelope is composed.

Fig. 53 represents a photograph obtained during the eclipse of 1889, and I am indebted to Professor Pickering for the opportunity of reproducing it. It seems that on this occasion the corona was not portrayed to quite the same extent that it has exhibited in certain other eclipses. The peculiar character of the coronal light is, however, clearly manifested. It must be at once observed that whatever may be the material of which the corona is composed, it is much more unsubstantial than that constituting the chromosphere or the prominences which lie beneath it. On the occurrence of the eclipse of January 1st, 1889, a number of astronomers betook themselves to California, and occupied stations situated in the narrow path traversed by the shadow of the Moon as it swept over the Earth. Under the most favourable circumstances a total eclipse may last for as long as four or five minutes at any particular station. The eclipse of January, 1889, did not offer equal advantages to the observers, for it only lasted about a minute.

In another respect also the eclipse now under consideration was rather disadvantageous from the astronomical point of view. It took place at a time when there was but little solar disturbance as indicated by Sun-spots. The various envelopes surrounding the Sun appear to undergo excitement in sympathy with the periodic development of Sun-spots. Unusual ejections of prominences would seem to be synchronous with epochs at

FIG. 53.—THE CORONA OF JANUARY 1st, 1889.

which the corona displays its long branches with unusual splendour. At the beginning of 1889 it happened that the Sun was in comparative quiescence, so that the corona observed in that year was of a kind appropriate to a period of Sun-spot minimum. It was, however, quite distinct enough to reveal the two fundamental features which characterise most coronal pictures. The polar streams were seen to be curving away uniformly from each pole of the luminary, while at the solar equator a vast extension of the pearly light was exhibited, which seems to be quite as characteristic as the phenomena presented at the poles.

Drawings made at this time by Miss L. Todd show clearly that the equatorial extensions were but parts of great equatorial projections extending on one side to three times the diameter of the Sun, and on the other side to twice the same length. There is an excellent account of the various observations which were made by the party from the Washington University Observatory, in *Nature* for December 24th, 1891. Both the photographs and the drawings agree very well with those obtained by the members of Professor Pickering's expedition. Professor Engler devoted himself specially to the study of the corona, and produced a drawing in which the polar streamers are exhibited with great distinctness, as are also the equatorial projections. Professor Pritchett, who was in charge of the party, describes the brilliant appearance shown by the filaments at the poles, where they were radial in direction. They then gradually merged into the equatorial streamers as the eye passed from the pole to the equator.

A composite photograph of the corona is also very instructive, illustrating as it does those critical points around the margin of the Sun where the radial streamers near the pole cease, and the equatorial streamers begin. In a discussion of the whole subject, Professor Todd deduced the following results from the various photographs taken on January 1st, 1889. It is first to be noted that the axis of symmetry of the corona does not coincide with the axis about which the Sun rotates, the position of which is indicated by the solar spots. It also appears that the corona must be of a more complex character than has hitherto been supposed. At least three distinct phenomena appertain to it. First, there are the polar rays, or rather those filamentous parts which are seen most conspicuously in the vicinity of the solar poles. There is, secondly, what Professor Todd calls the inner equatorial corona, the lower parts of which resemble an outer solar atmosphere; and there is, thirdly, the outer corona, which is composed of long equatorial streamers, not always depicted upon the photographs, but often visible to the naked eye. It also appears that the polar corona is composed of nearly straight rays which diverge from areas in the vicinity of the solar pole. A large percentage of the total coronal light is dispensed from the inner equatorial corona. The streamers of which it is composed are not nearly so well defined as those about the poles; many of them appear to possess distinct curvature. On the east side of the Sun they converge, while on the west they diverge. The presence of certain large prominences at thirty-five degrees of solar latitude suggests relation between the protuberances and the corona.

One further inference of considerable importance has been drawn by Professor Todd from an examination of the photographs in 1889, that the epoch of greatest coronal extension in the equator coincides very nearly with the epoch of minimum Sun-spots. Professor Tacchini, from an examination of Mr. Barnard's negatives, came to the conclusion, already expressed by Mr. Ranyard in 1879, that the regions of greatest protuberances are also the regions where the corona is best developed. In the eclipse of August 29th, 1886, some features of the corona were shown differing markedly from those of the eclipse of 1889 which we have been just considering. It seems as if some jets of intensely luminous material were projected through the corona to a distance of twice and a half the diameter of the Sun. The resulting curious hook-like structure is represented in Fig. 61, and close beside it is a picture of a somewhat similar structure witnessed in the eclipse of 1871.

A total solar eclipse took place on December 22nd, 1889, and was utilised in a very efficient manner by a party organised by Professor Edward S. Holden, director of the Lick Observatory. I especially refer to the report of Messrs. S. W. Burnham and J. M. Schaeberle, who, by the generosity of an American citizen, were equipped with everything necessary for an expedition to Cayenne, in French Guiana, which was found to be a suitable locality for the purpose. The telescope employed was a 6½-inch equatorial of excellent quality, the work of Messrs. Alvan Clark and Sons, the focal length being about 76 inches. Though the objective was not specially intended for photographic use, yet it was found to give good definition when the position of the plate was a little outside the visual focus. The position of the focus adapted for photographic purposes was carefully determined by making a number of star exposures on the same plate at various distances from the visual focus, differing from each other by about one-sixteenth of an inch. On the development of the plate the star which gave the best image showed the most suitable position of the plate.

To utilise the minutes of the eclipse to the utmost possible extent, periods of exposure were decided upon beforehand of two, five, seven, ten, and twenty-five seconds respectively; the aperture of the object-glass being reduced to three inches. The method employed for giving the exposures was to draw the slide and then lift a light black velvet flap hinged above the object glass. This could be controlled by a string from the eye-end, so that it would drop down when the cord was released. The phase of totality not having concluded when the last of the plates arranged for the experiment had been exposed, another plate was introduced. However, totality was over a little sooner than was expected, and the flash of sunlight fogged the plate and removed all traces of the corona though some other features of interest were left. The whole time of the eclipse was thus occupied in photographic work, nor could the observers spare any time for eye-observations of the corona, except at that first instant when at the commencement of totality, the outburst of the coronal light announced to them that the moment for taking the photograph had arrived.

Besides the apparatus we have named, a Dallmeyer portrait lens of six inches aperture was used on the same occasion. The object of this instrument was to obtain the greatest possible extent of the corona, even at the sacrifice of certain details of the structure in the brighter parts near the Sun's limb. Its full aperture was employed, and on this account, as well as by reason of the shorter focus of the portrait lens, much fainter objects could be represented by its means than was possible in the picture taken with the other telescope, in which the representation of faint detail was sacrificed for the purpose of obtaining exceptional accuracy in those parts of the corona which the instrument was capable of exhibiting. The exposures given were respectively five seconds, ten seconds, fifteen seconds, twenty and twenty-five seconds.

During the longer of these exposures the observers were at liberty to obtain naked-eye views of the corona. They remarked that the brightness of the light was so great that it did not seem impossible, by observing proper precautions, to render the corona visible even during ordinary sunshine. We are told that the terrestrial objects around were easily apparent at all distances, and the observers had the general impression that, for purposes of illumination, the radiance from the corona was much more effective than the radiance from the full moon. They described the appearance presented as somewhat resembling that obtained in ordinary sunlight by looking through a neutral-tinted dark glass. Terrestrial objects were well defined, but devoid of colour. In addition to the two photographic refractors whose use has been explained, an eighteen-inch silver-on-glass reflector was also employed; and as some illustration of the energy with which the observers did their work, we may mention that they brought this mirror to Cayenne without either tube or mounting, and improvised appliances on the spot, which seemed to have answered wonderfully well considering their necessarily rough character.

When the day of the eclipse arrived, the weather looked badly; and half an hour before the phase of totality commenced, it was found necessary to cover up the instrument to screen it from the drizzling fog. However, good fortune favoured the observers; twenty minutes before totality the sky began to improve, and when the critical time arrived, it appears that, so far as weather was concerned, the conditions were as satisfactory as could be wished. The dry plates employed on this occasion were those made by the Seed Company. Great care was taken in the development to treat each plate on its own merits, so as to bring out the most favourable result. It presently appeared that the exposures were almost all too long; indeed, with a quick lens like the Dallmeyer a rapid instantaneous shutter would have been most suitable for getting the fine detail near the Sun.

It has been generally found that the best representations of the corona have been obtained with plates which have been given a very brief exposure, and upon which the utmost care has been bestowed in a slow process of development. It has to be remembered that the background of the sky is still to some extent luminous, even at the central moment of a total eclipse. If the exposure be protracted, this extraneous luminosity is represented, and it seems that the gain to the corona by a long exposure is far more than counterbalanced by the injury to the plate produced by the luminosity of the sky. The most satisfactory plates are those in which the exposure has been so brief that the sky parts of the plate are perfectly transparent. No doubt, as Professor Holden has pointed out, the best results are to be anticipated only when photographs of the corona have been obtained at a station with an altitude 15,000 or 20,000 feet above the sea-level. The five negatives that were procured on this occasion have, however, been made to yield excellent results. They show clearly the polar rays of the corona, and also the form of the equatorial portions.

Professor Holden has compiled a very valuable report on the photographs of the corona taken during the eclipse of January 1st, 1889. An abstract of this paper appeared in the Monthly Notices for April, 1889, in which were given the views here reproduced. A diagram of the solar corona was prepared from a series of photographs obtained by Professor Barnard of the Lick Observatory. His apparatus was a very simple one, consisting of an ordinary achromatic telescope of 3 inches aperture and 49 inches focus, the aperture being reduced for the purposes of this observation to 1¾ inches. The object of this diagram (Fig. 54) was to represent the various details of the corona, no attempt of course being made to give any pictorial representation. How copious was the detail obtained is evidenced by

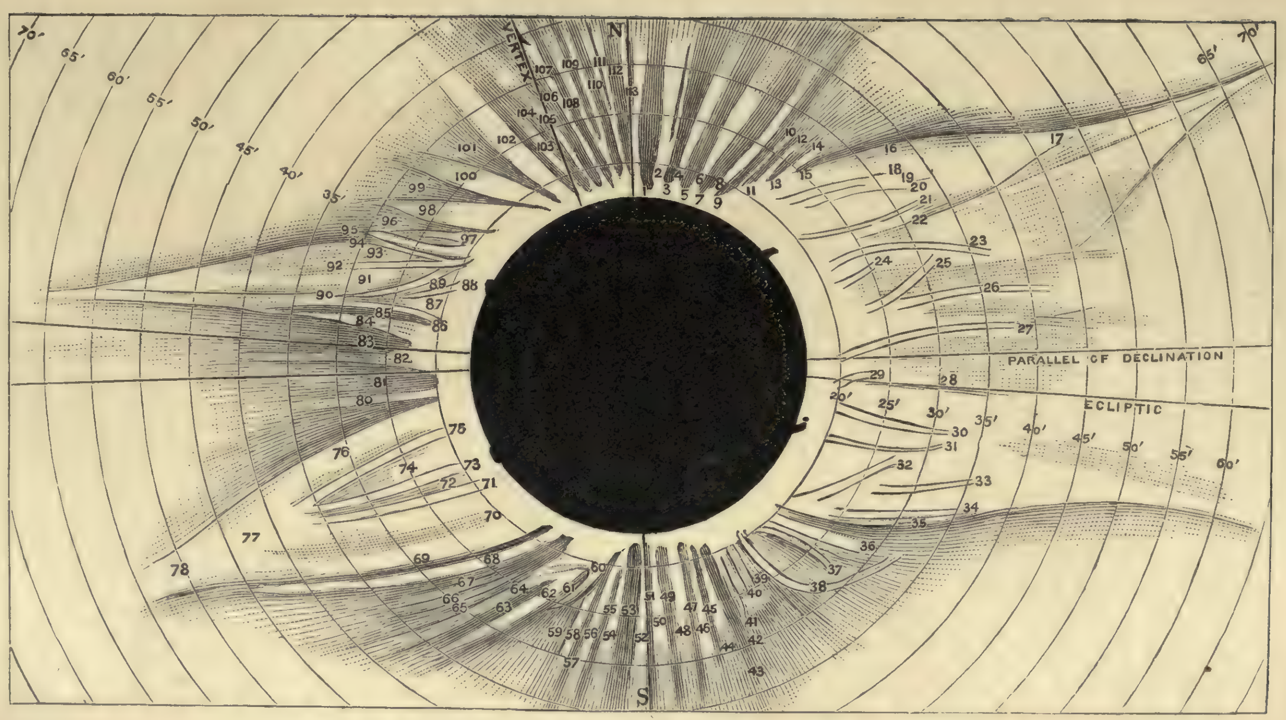


FIG. 54.—PROFESSOR HOLDEN'S DIAGRAM OF THE CORONA.

the fact that Professor Holden found it necessary, for purposes of reference, to mark the different features by distinguishing numerals, 113 being the total number.

What we describe as the corona is really the conjoint effect of three phenomena, the difference between which must be carefully borne in mind. There is first of all apparently some influence due to simple diffraction of the Sun's light round the edge of the Moon. This, of course, is a purely optical phenomenon, not implying any special appendage to the Sun. Its nature may be illustrated in many ways by familiar observation; for instance: Let the observer so stand that a lighted street lamp appears beside the pillar of a building at night, then, by moving the eye so that the lamp is just eclipsed, indications of its light will be still clearly manifest round the edge of the pillar. This is a diffraction phenomenon, and something of this kind must take place when the dark, airless globe of the Moon is interposed between the Earth and the Sun. Of course it will be understood that when we speak of the corona it is not a mere optical phenomenon of this kind that we refer to. It is, however, necessary to be on our guard against attributing an independent existence to something which, after all, might not relate to the solar envelope at all, but might be explained by a known property of light.

The corona is certainly not to be explained as a consequence of diffraction. When the globe of the Moon is placed centrally over the Sun, it will be quite obvious that any effect which diffraction would be competent to produce must be symmetrical all round the globe. Such parts of the corona as are not symmetrical must therefore be referred to the action of agents wholly different from those by which a mere diffraction phenomenon takes its rise. Now there are two elements of coronal structure which are obviously unsymmetrical—the polar rays and its equatorial extensions.

The index map of the corona in Fig. 54 will serve as a convenient means for referring to its features. The several concentric circles which surround the dark disc of the moon indicate the distances from the solar centre. Thus the circle marked 40 mins. means the circle whose radius was 40 mins.

distance from the centre of the black disc. At the south-east, the corona stretches to a point 55 mins. from the Moon's centre. At the opposite part of the limb the north-west wing goes as far as 70 mins. The south-west wing can be followed as far as 60 mins., and the north-east can be traced to 55 mins. The several polar rays, or at least the more important of them, extend 25 mins. or 30 mins. from the Moon's centre, and the longest ray marked No. 2, appears to be 36 mins. long in Mr. Barnard's best negative. Inside the circle of 20 mins. there is but little to be seen except four protuberances, and it is impossible not to note that these protuberances occupy distinctly characteristic positions relatively to the equatorial extensions of the corona. On the next page (Fig. 55) we exhibit the diagrams of the outer corona from photographs taken by Messrs. Ireland and Lowden. These show wing-like extensions of the corona reaching far beyond the limits of the other picture; indeed, out to 135 and 165 minutes respectively.

Professor Holden's careful discussion of the evidence seems to point to the conclusion that the so-called polar rays are not peculiar to the polar regions of the Sun, but that rays of an analogous character are developed all round the margin of the luminary. In the case of those in the equatorial regions, the rays are projected against the bright light of the equatorial structures. Hence they cannot be so readily discerned as in the polar regions, where they are projected directly against the sky.

As to the nature of the corona, there is no small variety of opinion among astronomers. Professor Schaeberle, of the Lick Observatory, has been led, by the eclipse of December 22nd 1889, to the belief that the corona is probably due to light reflected from streams of matter ejected from the Sun along lines normal to the surface. The forces by which these ejections are produced appear to be most active about the middle portion of the north and south Sun-spot zones. As the Sun is differently placed with regard to the observer, varied changes in perspective account, according to this view, for the different aspects in which the corona is presented. To test in some fashion the capabilities of this supposition for representing the actual

FIG. 55.—DIAGRAM OF THE CORONA.

phenomena which the corona presents, Professor Schaeberle arranged a number of wires projecting from a ball along zones in the same relative positions as those frequented by Sun-spots. This ball was placed in a beam of parallel light, and its shadow was projected on a screen. By varying the position of the ball it seems that a variety of forms, closely resembling those frequently seen in the coronal structure, could be produced. (Figs. 56—69.)

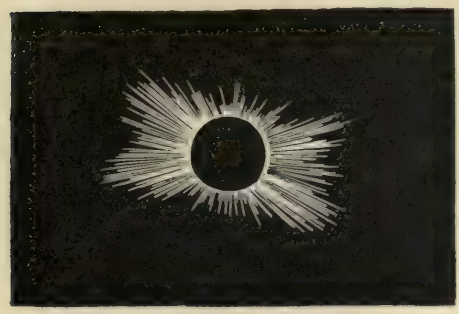


Fig. 56.—Schaeberle's Theory of the Corona.

Another theory of the coronal structure has been brought forward by Professor Bigelow. Here electricity is invoked to explain the phenomena which are produced. The coronal matter is supposed to consist of materials driven away from the Sun by the action of electrical repulsion. It seems that on this principle it is possible to explain, not only the polar rays of the corona, but also the well-marked equatorial appendages. In a later publication, Professor Bigelow gives some further developments of his investigations of coronal forms. In this inquiry he determines the position of that axis about which the corona is symmetrically adjusted, and it appears to be inclined at an angle of four and a half degrees to the axis of rotation of the Sun, as determined from Sun-spot observations.

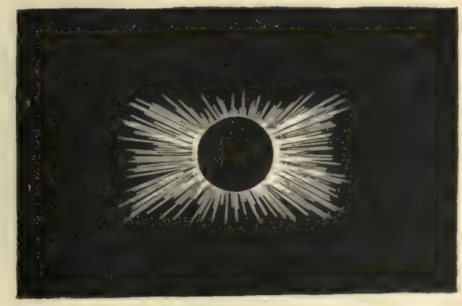


Fig. 57.—Schaeberle's Theory of the Corona.

Among the few exact measurements that have been made of the actual light radiated by the corona, we may mention those of Professor Thorpe during the solar eclipse of 1886. It appears that the alteration in the intensity of coronal light at different distances from the Sun's limb does not vary precisely according to the law of inverse squares. The brightness, in fact, of the corona at considerable elevations is greater than it would be if that law were observed. The most brilliant part of the corona which was measured lay at a distance of 1·55 solar semi-diameters from the centre of the Sun, the lustre there being about one two-hundredth part of that of the surface of the Moon. The lustre of the most remote part of the corona at a distance of 3·66 solar diameters was about one eight-hundredth part of the brightness of the Moon.

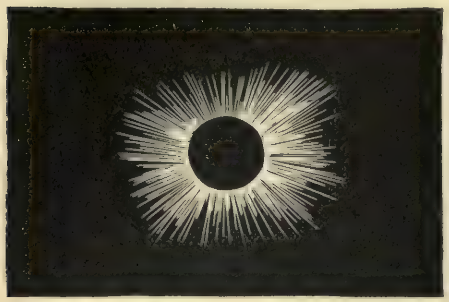


FIG. 58.—MECHANICAL THEORY OF THE CORONA.

The spectroscope has also been applied to the examination of the character of the light emitted from the corona. There is a certain line in the green part of the spectrum marked 1474 on Kirchhoff's scale. This line is seen to consist of a pair when a sufficiently high dispersive power is employed. One of these lines has been attributed to the element iron; the other line of the pair belongs to the characteristic coronal element, a substance at present otherwise unknown. In 1871 Janssen

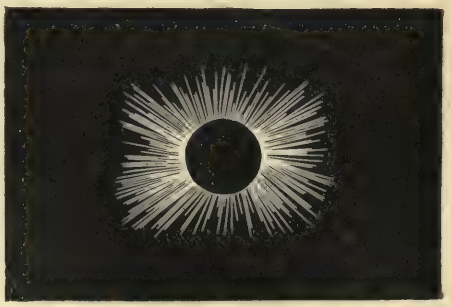


FIG. 59.—MECHANICAL THEORY OF THE CORONA.

discovered that besides the celebrated one marked 1474 there are other bright lines in the coronal spectrum, among which it should be specially noticed that the hydrogen lines are occasionally to be seen.

Such is an outline of the facts known to us with regard to the corona; and it must be admitted that our information is at present of a somewhat meagre description. We can only hope that the attempts to photograph the corona without having to wait for a total eclipse may ultimately prove successful. Doubtless many of our perplexities would vanish if a series of observations taken at brief intervals were certainly available. We might then expect to gain information regarding the changes in the corona, which it seems absolutely certain are in progress. We might expect, too, that some satisfactory evidence might be forthcoming as to the actual character of the material to which the coronal light is due.

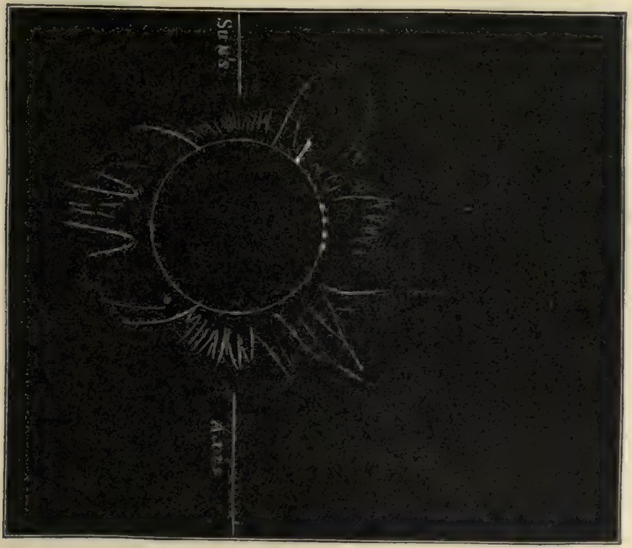


Fig. 60.—Outline of Corona.

(*From "Knowledge."*)

It should be remarked that there is one solar phenomenon, often conspicuous, which has been thought to be connected with the solar corona. The zodiacal light is a beautiful phenomenon frequently to be witnessed in our own latitudes, and often presenting a striking appearance in equatorial regions. It is best seen in these countries either in the spring months just after sunset, or in the autumn just before sunrise. The phenomenon would, perhaps, be more generally noticed at sundown were it not that the casual observer is liable to mistake the zodiacal

FIG. 61.—HOOK-LIKE PROJECTIONS IN THE CORONA.

(From "Knowledge.")

PLATE VIII.

CASSELL & COMPANY LIMITED LITH LONDON

ZODIACAL LIGHT AS SEEN AT PALERMO, 1st APRIL (8.40 p.m.), 1872.

(From a Sketch by Prof. Piazzi Smyth.)

light for the ordinary twilight that marks the departure of the luminary.

When the zodiacal light is recognised after sunset, it appears like a mighty projection of luminous matter from the west, extending to a considerable altitude, as is well shown in Plate VIII. which is taken from a painting by Professor Piazzi Smyth. It is, indeed, impossible, when we consider this object in its varied aspects, to refuse assent to the belief that what we are looking at must be a vast mass of luminous hazy material surrounding the Sun; in ordinary daylight this material is not visible, because the bright light from the Sun completely extinguishes such feeble radiance as the zodiacal light possesses. But when the luminary has itself disappeared below the horizon, so that its direct beams are cut off, then the light above the horizon may be clearly seen, notwithstanding its faintness. In a similar manner, just before sunrise, at the proper season, the observer looking towards the east will see as the herald of the approaching day, not only the dawn, but also this pearly light ascending above the eastern horizon.

These observations demonstrate that there must be a mighty volume of glowing luminous material surrounding the Sun. Now we might reasonably expect that in a total eclipse this matter, which we frequently see in the zodiacal light, would become visible when the brilliant light was intercepted by which its feeble lustre was generally extinguished. Such a display would evidently resemble that which we have already been describing in this chapter under the title of the Solar Corona. We cannot then feel surprised at the belief that the zodiacal light and the corona must be connected. At the same time, it must be observed that the extension of light indicated by the zodiacal phenomenon vastly transcends that of the corona as ordinarily described. It is not unusual for the zodiacal light to attain an altitude of thirty degrees above the horizon—that is to say, an extent sixty times the diameter of the Sun. But even the most extravagant estimates of the dimensions attained by the corona in a total eclipse fall far short of what such figures indicate. If the corona and the zodiacal light were really the same phenomenon, we should then be forced to the conclusion that what we saw during total eclipses was only an inconsiderable fraction of the total volume of luminous material visible in the zodiacal cone. In fact, it seems as if the two phenomena must be accounted for on different grounds; so that we gain but little help in the comprehension of the mysteries of the corona by likening it to the zodiacal light. We can only hope that ere long further information on the subject may be forthcoming.

CHAPTER XI.

SOLAR AND MAGNETIC PHENOMENA.

IT seems now impossible to doubt that seasons of exceptional activity on the Sun exercise an influence of a specific kind on the Earth. The matter is still, it is true, affected with no little uncertainty; but having given the warning that we are even yet by no means clear as to the nature of the connection between solar activity and terrestrial magnetism, we shall set forth an outline of what has been ascertained on the subject.

It is known that the Earth acts in some respects like a magnet; indeed, it is on the attraction of this great terrestrial magnet on the needles of the mariner's compass that the utility of the instrument depends. The compass of the navigator is, however, but a coarse apparatus for the purposes of scientific measurement. It has to be constructed in such a robust fashion as to stand the exigencies attendant on its position on shipboard. When we desire to measure the magnetism of the Earth with the delicacy and precision which is demanded by the needs of science, the rough-and-ready compass so invaluable to the navigator has to be replaced by an instrument of precision. In our magnetic observatories magnetised bars are suspended so as to record with the utmost delicacy the precise state of terrestrial magnetism at the moment. Every precaution is taken to render the slightest oscillations of the bar perceptible. The details of the suspension are arranged with great care; and in order to render movements visible, even though they are of little more than microscopic magnitude, a mirror is attached to the needle. On this mirror a slender beam of light is projected from a lantern, and the bright spot is reflected thence to a screen. In order to preserve a continuous exhibition of the indications of the needle, the screen is of photographic paper, and by this means the state of the Earth's magnetism at any hour is recorded in a form which can be preserved for subsequent reference and comparison. We ought to add that iron is scrupulously avoided in any building in which magnetic instruments are housed. Even the locks on the door must be made of some metal other than that on which magnetism exercises so specific an influence.

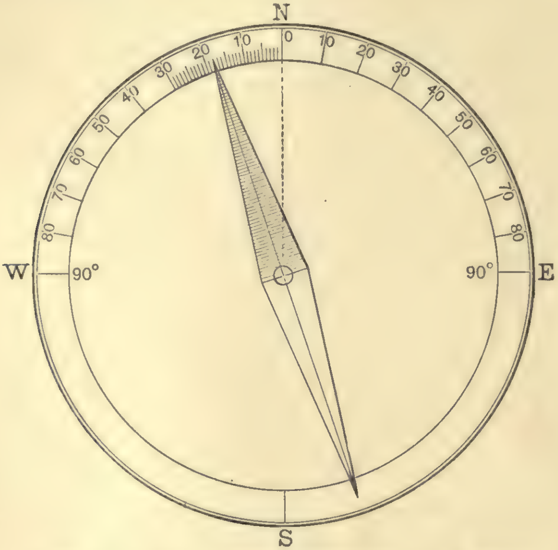


Fig. 62.—The Magnetic Declination at London.

The magnetic declination is illustrated in the adjoining figure, which represents the position of the needle at London (1893). In the case of a magnetic storm, the needle undergoes vibrations, for the accurate measurement of which the photographic methods are so useful. Besides the instrument which measures what we call the magnetic declination—that is to say, the angle of deviation of the magnetic needle from the true meridian—there is another instrument for measuring a different element of the Earth's magnetism which must be attended to. It is true that the element now referred to has no practical application in the use of the compass, but it is of the utmost importance in the theoretical study of terrestrial magnetism. Magnetic observatories contain what is called the dip-needle, by which the direction of a needle revolving around its centre of gravity in a vertical plane can be measured. To the two elements of declination and of dip a third must be added—viz., the absolute intensity of the magnetic force. It is the function of a fully equipped magnetic observatory to record continuously these three elements of declination, of dip, and of intensity.

It has long been known that the magnetic declination was liable to change. The value of this element varies not alone at different points of the Earth, but even at any stated locality it undergoes continuous changes with lapse of time. Indeed, these fluctuations in the declination are sufficiently important to engage the attention of the mariner, who has to observe that the difference between the magnetic pole and the true pole must be continually corrected up to date. But, besides the larger and more important changes, the refined observations of our delicate instruments render minute angular displacements perceptible, which would be wholly invisible on the comparatively rude scale of even the best mariner's compass. We are thus taught that the great changes in the magnetic elements are only the more conspicuous of a host of kindred phenomena. The magnetic needle is in incessant movement of one kind or another, and the study of its minute changes has opened up a field of instructive information. We learn, for example, that one distinct fluctuation in the declination has a diurnal period; and we also note that so-called magnetic storms frequently occur in which the magnet is thrown into what can only be described as a state of violent oscillation by contrast with the very deliberate movements by which it is usually affected.

It generally happens on the occurrence of a magnetic storm that the three elements are simultaneously affected. This proves that occasional disturbances arise from some cause or other by which the magnetic state of the Earth is thrown into a tremor. Now though these tremors are quite incapable of producing any effect that appeals immediately to our senses, and though, in fact, they are too delicate to be measurable even by the best constructed mariner's compass, yet the photographic records of the instruments in magnetic observatories yield all the information we want. They show the time that the magnetic storm commenced, and the time when it ceased ; they also demonstrate what might have been otherwise anticipated, that a magnetic storm is no mere local phenomenon, but that the whole Earth is affected by such an occurrence. The same influence which makes the magnetic instruments tremble at Greenwich or at Kew, affects also the instruments in France or Germany, India or Australia. It is then plain that the agent, whatever it may be, by which magnetic storms are produced, stirs our globe as a whole.

A frequent concomitant of magnetic storms sometimes appeals to our sense of vision in a very striking manner. I allude to the Aurora Borealis. For though the nature of its connection with the terrestrial magnetic condition is still very obscure, it is found that whenever the Aurora is especially brilliant and remarkable, magnetic storms of unusual vehemence are, generally speaking, in progress at the same time. M. Pupin's experiments have a remarkable suggestiveness in connection with the aurora. In the adjoining page we have reproduced some of M. Pupin's drawings of the electrified poles. It certainly does seem as if we had in these pictures of known electrical discharges representations of phenomena resembling auroral discharges. It is also instructive to compare the first of these pictures with Schaeberle's photograph of the corona as seen on April 16th, 1893. (Fig. 64.)

It appears quite certain that the phenomena of terrestrial magnetism are connected with the existence of electric currents which circulate round the Earth, and it is clear that these currents are connected with the movements of the Sun. In fact, there is a diurnal period in certain magnetic phenomena which would hardly seem to be explicable on any other supposition save that Sun-heat is an important factor in the case. It is therefore not surprising to find that when a profuse crop of Sun-spots shows the Sun to be in exuberant activity, then the action of this exceptional excitement of the Sun produces a corresponding influence on the magnetic state of the Earth. It

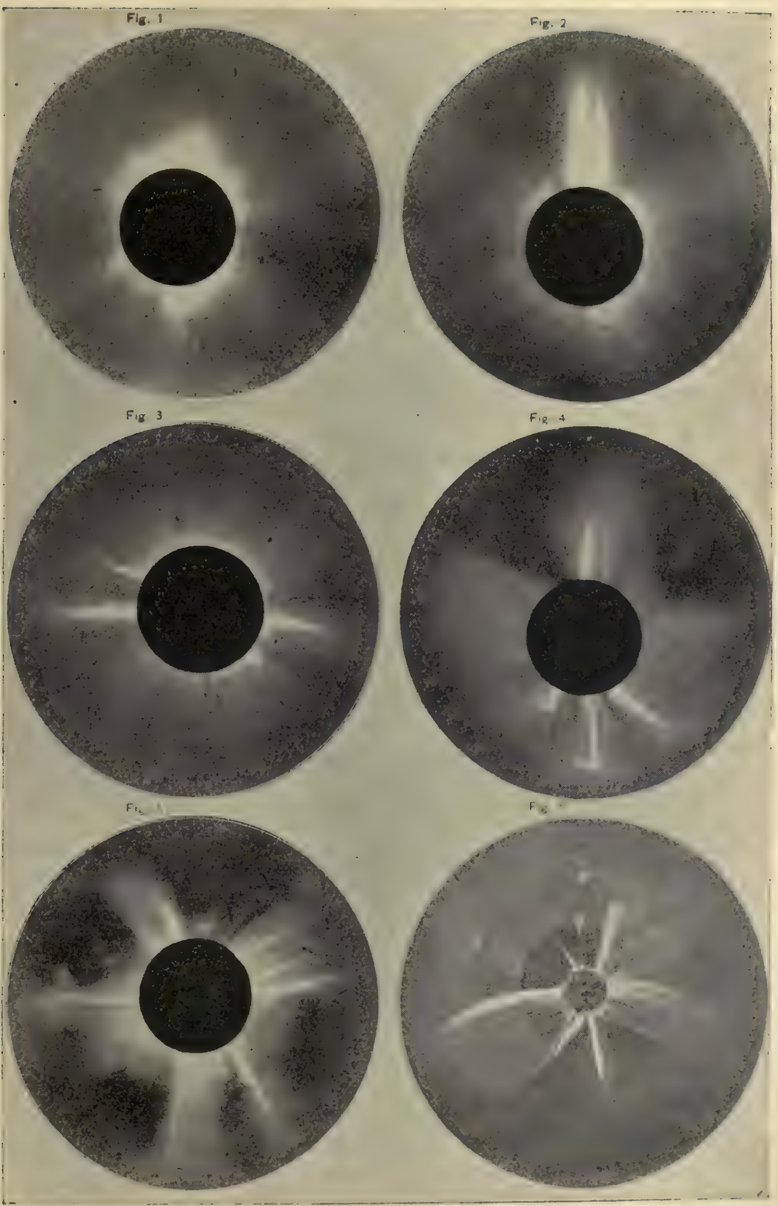


FIG. 63.—CORONOIDAL ELECTRICAL DISCHARGES (*Pupin*).

is interesting to note that there is a large and valuable body of evidence available to demonstrate that there does exist some sympathy between periods of solar agitation and periods of excited terrestrial magnetism.

A recent instance of the connection between Sun-spots and magnetism is given by Mr. E. W. Maunder in *Knowledge* for May 2nd, 1892. He there describes the magnetic storm, as indicated on the self-recording instruments at Greenwich, which appeared to attend the outbreak of the great spot on the Sun in February, 1892. In the paper referred to Mr. Maunder makes the important remark that: "In a period of nearly nineteen years, therefore, we have three magnetic storms which stand out pre-eminently above all others during that interval. In that same period we have three great Sun-spot displays—counting the two groups of April, 1882, together—which stand out with equal distinctness far above all other similar displays. And we find that the three magnetic storms were simultaneous with the greatest development of the spots." It would be incompatible with the probabilities of the case to doubt the reality of the connection here suggested.

Much other testimony of a similar kind could be adduced. A paper on "Sun-spots and Magnetic Perturbations in 1892" is given in *Astronomy and Astrophysics* for January, 1893. In this the distinguished solar observer, M. Riccò, has compared the evidences of solar activity yielded by his own observations with the facts of magnetic perturbations taken from the photo-magnetographs of the United States Naval Observatory. Before the comparisons be made, it is necessary to note that since a spot has some considerable duration, it is desirable to take as the epoch of its appearance the hour of its passage across the Sun's central meridian—that is to say, the epoch when it passes across the meridian drawn from the centre of the Sun to the celestial pole. M. Riccò collected eleven cases in the year 1892 in which a spot of important size had crossed the central meridian. In some he refers, of course, to the same object seen at successive returns. Thus the great spot of February 12th, which passed the central meridian at 4 a.m. on that day, passed the central meridian again on March 10th

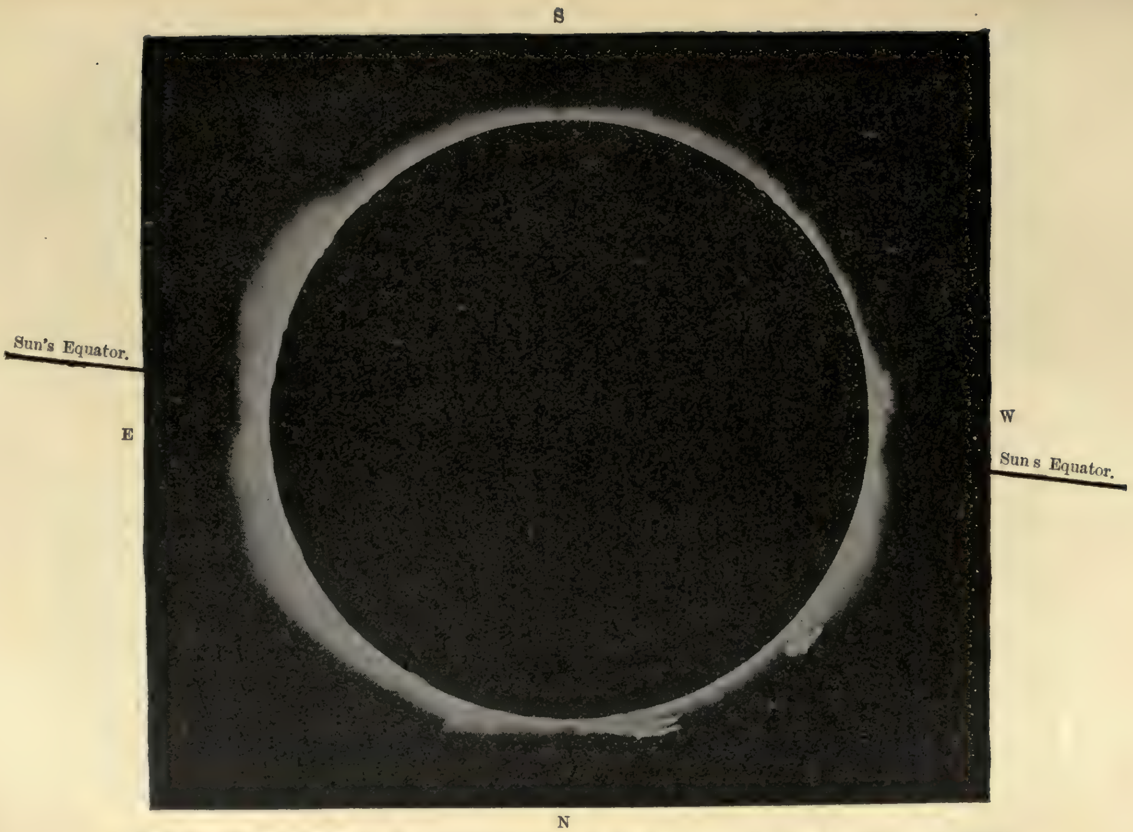
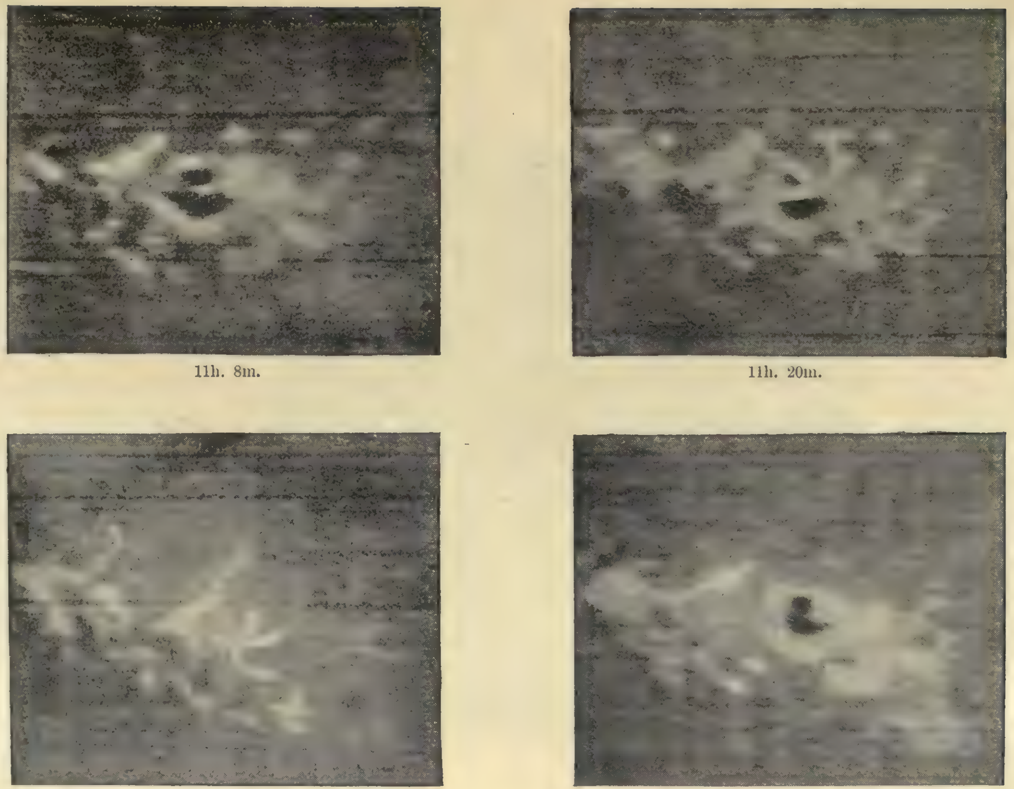


FIG. 64.—SCHAEBERLE'S PHOTOGRAPH OF THE CORONA OF APRIL 16TH, 1893.

at 2 p.m. But out of the eleven cases referred to, there are no less than seven instances in which the passage of the spot over the central meridian was followed by a terrestrial magnetic disturbance. To refer again to the great spot of February 12th, it would appear that forty-five hours after it crossed the central meridian, a magnetic storm of extraordinary vehemence was manifested. At the next return of the same object on March 10th, a magnetic storm described as "very strong" was also recorded by the photo-magnetographs, the interval being again forty-five hours. What makes the observations the more significant is the fact that, generally speaking, the larger the spot the more vehement appears to have been the magnetic storm. It is also to be noted that the interval of time between the transit of the spot across the central meridian of the Sun and the culmination of the magnetic storm, appears to have been practically constant, amounting to two days.

In connection with this subject it is evidently desirable to watch for the occurrence of exceptionally vehement solar outbreaks, and to compare them with the magnetic records. We are able to do this with special advantage in the case of the solar eruption illustrated in Fig. 65, which gives four pictures taken by Professor Hale with the spectro-heliograph on July 15th, 1892. Professor Hale discusses the question as to the possibility of the outbreaks on the Sun, of which one is here represented, being associated with any magnetic phenomena. He concludes that "the perturbations of terrestrial magnetism seem to synchronise closely with the activity of various groups of spots and faculæ indifferently situated in various parts of the solar surface."

Several instances have been recorded, notably a famous one by Messrs. Carrington and Hodgson, in 1859, where a sudden development of luminous material on the Sun was associated in the most marked and unmistakable manner with the outbreak of a magnetic storm of wholly exceptional abruptness and intensity. The frequency of such coincidences leaves no room for any reasonable doubt as to the reality of the connection which they indicate. At the same time it is right to add that it must not be assumed that solar manifestations of the kind



11h. 8m.

11h. 20m.

11h. 47m.

2h. 0m.

Fig. 65.—SOLAR ERUPTION, PHOTOGRAPHED JULY 15th, 1892, WITH THE SPECTROHELIOGRAPH OF THE KENWOOD OBSERVATORY.

referred to are necessarily accompanied by terrestrial magnetic phenomena. For instance, on June 17th, 1891, M. Trouvelot observed a luminous outburst on the Sun, apparently of a kind analogous to those which had been previously found to be attended by magnetic outbreaks. The spectroscope showed that the object Trouvelot observed arose from a centre of eruption, whence glowing bombs were projected far above the chromosphere, where they seemed like brilliant globes on the darker background. The following morning the intensity of the eruption was still unexhausted, and then it gradually declined until it disappeared in the course of the afternoon. The circumstances were such as to lead Trouvelot to inquire whether any magnetic perturbation took place simultaneously. But no evidence of the kind was to be found in the records obtained at the Kew Observatory, and although there were some apparent indications at Greenwich of a slight magnetic storm at the

FIG. 66.—THE SOLAR PROMINENCES AND FACULÆ OF APRIL 16TH, 1893 (*Hale*).

time required, yet it was evident that no disturbance took place of the vehemence which might have been expected.

The striking picture of the solar prominences and faculæ in Fig. 66 was obtained by Professor Hale from photographs taken by his spectro-heliograph on April 16th, 1893. This day was chosen as it was the occasion of the total eclipse, and it seemed interesting to compare the activity of the Sun as indicated on its disc with the condition of the corona as ascertained by the eclipse observers. All such inquiries have a special significance in connection with the study of the possible association between magnetic phenomena and the activity of the Sun.

A new phase of the question has recently been opened up by the address delivered at the Royal Society's Anniversary on November 30th, 1892, in which Lord Kelvin criticised adversely the theory that terrestrial magnetic storms were directly due to magnetic waves emanating from the Sun. The President of the Royal Society pointed out the fundamental character of the difficulties attending the belief that any influence of the kind supposed is directly exerted by the Sun upon the Earth. It appears that under ordinary circumstances the variation in the intensity of the Earth's magnetism in the course of a storm is as much as one four-hundredth part of the undisturbed terrestrial magnetic force. In extreme cases, however, the variation may amount to as much as one-twentieth of the Earth's normal magnetic force. The chief difficulty is to imagine how the Sun could be a sufficiently powerful variable magnet to produce at the distance of the Earth changes so great as those which are actually observed to take place.

Consider the work which must be done if the Sun is to produce such a disturbance. The instance cited by Lord Kelvin is that of the magnetic storm of June 25th, 1885, concerning which Professor W. G. Adams has given particulars in his paper of June, 1891, in the Philosophical Transactions. At eleven places all over the world the horizontal force increased and fell, the entire time of the phenomena occupying altogether about eight hours. The horizontal force at certain of these places was increased by nearly one-thousandth above, and then sank to nearly one-thousandth below the normal value. It can be shown that for such changes as these to have arisen from any action within the Sun, the agent must have exerted a horse power about 364 times as great as that of the entire solar radiation. "Thus," says Lord Kelvin, "in this eight hours of a not very severe magnetic storm, as much work must have been done by the Sun in sending magnetic waves out in all directions through space as he actually does in four months of his regular heat and light. This result is, it seems to me, absolutely conclusive against the supposition that terrestrial magnetic storms are due to magnetic action of the Sun; or to any kind of dynamical action taking place within the Sun, or in connection with hurricanes in his atmosphere, or anywhere near the Sun's outside.

This reasoning almost forces us to reject the opinion that solar activity is the cause of magnetic disturbances; and Lord Kelvin seems to suggest that the many instances in which an agreement has been noted between the period of exuberant Sun-spots and the periods of exuberance of magnetic storms must be classed as merely accidental coincidences. It seems, however, difficult to think that this can be the case. It is just possible that there may yet be some other explanation available which will admit of our retention of a belief in the connection between solar outbreaks and terrestrial magnetic storms, and yet will not require us to regard either as the cause of the other. Can it possibly be that the phenomena both on the Sun and in the Earth do not stand in the relation of cause and effect at all, but are each of them manifestations of some other influence of electro-magnetic waves on a vast scale sweeping through our system, and influencing the magnetic phenomena in the various bodies of which our system is composed?

CHAPTER XII.

THE MECHANICAL THEORY OF HEAT.

IT is impossible to understand the relations of the Sun to the system over which it presides without having made some acquaintance with that important department of modern science known as the mechanical theory of heat. I shall therefore set forth in this chapter a brief explanation of the fundamental principles on which this theory depends, and make such applications of it to astronomical phenomena as the subject naturally suggests.

In the first place, it is necessary to understand the methods employed in measuring quantities of heat; and here we should state that the conception of a quantity of heat is something of a very different kind from that which is understood when we speak of temperature. A thermometer shows the temperature of the body, but its indications are not really to be taken as measures of the quantity of heat which the body contains, though no doubt the two are connected. Take a pound of water, just at the freezing temperature of thirty-two degrees; then, if we warm that water until its temperature has risen one degree—viz., to thirty-three degrees—we shall, in so doing, have imparted to it one unit of heat. If its temperature be raised two degrees, then double the quantity of heat is required above that which would suffice to raise it by a single degree. Suppose, for instance, we were to raise this pound of water from the freezing point to the boiling point, its temperature would have to be exalted through one hundred and eighty degrees, in order to bring it from thirty-two to two hundred and twelve degrees; in other words, we should have to impart to the pound of water no less than one hundred and eighty units of heat. In like manner it will be readily understood that to raise two pounds of water through one degree, two units of heat are required; and similarly, to raise ten pounds of water through one degree, ten units of heat would be necessary. In general, the number of units of heat absorbed in raising a certain number of pounds of water through a certain number of degrees is simply the product of the number of pounds in the mass by the number of degrees through which its temperature ascends.

The conception of definite quantities of heat is of practical utility. When we buy a ton of coal what we are really purchasing is equivalent to a certain amount of heat. That ton of coal virtually contains a certain number of units of heat, for there are just a definite number of pounds of water which that mass of coal is capable of raising through one degree. The reflection that coal is merely heat in a compact form may facilitate our comprehension of what is meant by the mechanical equivalent of heat. Let us suppose that a certain quantity of coal is employed to heat the boiler which gives motion to a steam-engine; then for every pound of coal that is consumed in the furnace, a certain quantity of steam is generated, and an equivalent amount of power is rendered available.

No doubt, in the ordinary circumstances realised in practice, the efficiency of the engine falls considerably short of that which would be attainable were we actually able to utilise, for direct mechanical effect, all the heat that we know to be latent in the coal. We may, for our present purpose, suppose that engines have been produced of a far more perfect description than any which have as yet been constructed. It is indeed impossible to avoid great loss of heat, even with the most improved types of boilers and engines. To begin with, much of the heat from the coal escapes uselessly up the chimney; much of it passes directly into the atmosphere with the steam, or is expended in heating the water of the condenser. An ideally perfect engine would be one by which every portion of the heat that the coal actually contained would be applied to the production of mechanical effect. Although this end is far from being practically attained in the actual steam-engines of our workshops, yet the illustration suffices to make it clear that there must be some specific relation between the quantity of heat employed in the furnace and the quantity of mechanical effect which the engine produces.

We shall assume that the machine under consideration is employed in the most elementary form of mechanical effort. Suppose it to be applied to wind minerals up the shaft of a mine, from the bottom to the Earth's surface. The work that has to be accomplished will be measured partly by the depth of the mine, and partly by the weight of the minerals which have to be raised within a given time. It is clear that both the elements of depth and quantity must be taken into consideration. If there be two mines equally deep, but differing in the tonnage of mineral which has to be raised per diem, the horse-powers of the two winding-engines must be, in general, proportional to the tonnage to be lifted per diem. Or if the daily tonnage be the same for the two mines, but if one shaft be double as deep as the other, then the horse-power of the engine hoisting from the deep mine must be double as great as that which will suffice for the other. We define the foot-pound to be the quantity of work necessary to raise a weight of one pound through the height of one foot. Each engine is called upon each day to perform a duty which is expressed by a number of foot-pounds. From this point of view we now think of a steam-engine as a contrivance for performing foot-pounds of work, its boilers being supplied with coal, the heating power of which must, as we have explained, be expressed by units of heat. It thus appears that a certain number of units of heat, generated under the boilers, corresponds to so many foot-pounds of work done by the engine. The more foot-pounds of work that are obtained from each unit of heat employed, the more nearly does the engine approach perfection. But it has been ascertained that there is a limit of conceivable efficiency which even the most perfect engine could never transcend. Each unit of heat would, if every possible loss were avoided, perform precisely seven hundred and seventy-two foot-pounds of work. This is what is known as the mechanical equivalent of heat. I should, however, hasten to say that no efficiency approaching what is here implied could ever be realised, even in our most perfect steam-engines. By careful experiment, however, the equivalence between the unit of heat and the capacity to do seven hundred and seventy-two foot-pounds of work, has been thoroughly demonstrated. Conversely, too, it has been ascertained that when seven hundred and seventy-two foot-pounds of work have been perfectly applied, they can be made to produce a thermal effect precisely equivalent to that developed by one unit of heat.

Suppose that the descent of a heavy weight is made to impart motion to a revolving fan contained in a vessel of water. The motion of the fan through the liquid is necessarily accompanied by friction, which has the effect of warming the water, and, by a careful measurement of the exaltation in its temperature, the quantity of heat produced can be measured. In this experiment there are, no doubt, opportunities for the escape of heat, such, for instance, as in the cooling due to the radiation from the vessel. It is, however, invariably found that when all necessary precautions have been observed, and when due allowance has been made for all losses, the figures invariably shape themselves towards that final and most important result which asserts that each unit of heat is precisely equivalent to the performance of seven hundred and seventy-two units of work; or that, conversely, when seven hundred and seventy-two units of work are entirely devoted to the production of heat, the quantity that they will generate is precisely one unit. The significance of this conclusion is appreciable throughout the whole range of physical science. Its applications to astronomy are especially interesting and instructive, and its development on the grandest scale is presented in solar phenomena.

In the first place it must be observed that whatever is equivalent to work, in the sense in which we use the word, is thus demonstrated to be equivalent to heat. The exertion of raising a weight involves work; if now the weight be allowed to fall back towards the ground, it acquires during its descent a velocity which depends on the distance through which it has fallen. The work which has been, so to speak, communicated to the body by the act of its elevation is in the course of its descent transformed into work of a different kind, which is now manifested in the body by virtue of its motion. In this form the body possesses what is called by mathematicians kinetic energy. That the quantity of kinetic energy acquired by the fall is precisely equivalent to the work done in raising the body through a given height, will be obvious, if we suppose the body to be a perfectly elastic ball. In such a case, when it impinges upon a hard floor, the elastic body will rebound to the height from which it was let drop. Of course it is obvious that it can only be the energy possessed by the body in virtue of the velocity acquired during its descent, which suffices during the ascent to carry the ball back to the elevation from which it originally started. A perfectly elastic body does not, however, exist, so the experiment suggested is only partially practicable.

We see thus that kinetic energy can be stored in a body in virtue of the motion with which it is animated. The cannon-ball as it flies off possesses, in virtue of its motion, a kinetic energy derived from that which lay dormant in the gunpowder by which the motion was generated. It is essential to note that the quantity of energy contained by a moving body in virtue of its motion depends on the square of the velocity with which the body moves. This will be sufficiently evident from a consideration of the velocity acquired by a body through its descent by gravity from a known height. If a stone be dropped without any initial velocity from an altitude of 16 feet, it will possess a velocity of 32 feet per second by the time it reaches the ground. But if the stone is to attain a velocity double that amount—that is, of 64 feet per second by the time it reaches the ground—it must of course have started from some greater altitude. It would not have been sufficient to have dropped the stone from double the height employed in the former case. An elevation four times that which sufficed to give it a velocity of 32 feet per second would be demanded. If from a height of 64 feet a stone be let fall, it will be found to have a pace of 64 feet per second just as it arrives at the ground. Inasmuch as in the latter case we had to lift the stone to an altitude four times as great as that which sufficed in the former, the work done in the second operation was four times as great as that in the first.

We thus prove that if a certain quantity of energy must be expended in giving to a body a certain velocity per second, then four times the same quantity of energy will be required if the velocity produced is to be doubled. These considerations may be extended to many other cases beside those in which terrestrial gravitation has been the moving agent. We learn that whenever one body has a velocity of translation double that of another, the quantity of energy stored up in the former in virtue of this movement is four times as great as that in the latter. In like manner it can be demonstrated that a body moving in a straight line with a velocity of translation amounting to 300 feet per second will have an energy nine times that possessed by a body of the same mass which has a velocity of translation amounting to 100 feet per second. We may state the result more generally by saying that the kinetic energy of a body animated by a movement of translation is proportional to the product of the mass of the body into the square of its velocity.

In the rapid increase of the energy with the increase of velocity, we find a ready explanation of many astronomical phenomena. It will be instructive to consider the case of a shooting star, for it affords perhaps the most striking illustration that nature affords of the principles of the mechanical theory of heat. A shooting star is manifested by a streak of brilliant light observed to flash across the sky on a dark night. The appearance is really due to a small body which, moving through open space, suddenly impinges on our atmosphere, forces its way through the outer layers, grows luminous as it does so, and thus attracts attention. But it does not seem at first obvious why this process should be attended with such an evolution of light as is actually witnessed. We know that there is no evolution of light as the bullet of a rifle flies through the air; why, then, should a meteor be so brilliant? It is altogether a question of speed; the velocity with which one of these meteoric objects is animated greatly exceeds what could be produced by any piece of artillery.

We have been able to measure the speed of meteors. No doubt that speed varies not a little in different meteoric showers; but it is seldom if ever much less than about twenty miles per second. And we know that certain meteors attain a pace nearly double that which I have mentioned. But let us assume the velocity to be twenty miles a second, and examine whether we shall not be able to explain the light with which the meteor is attended. I believe that the greatest pace that can be imparted to the missiles from our modern weapons is about half-a-mile per second. It will be observed that this is the fortieth part of the velocity with which the meteor is animated. Let us further suppose that the projectile from the cannon and the meteor are equal in mass; and we can then estimate the relative quantities of energy they contain. We have shown that the energy which a moving body possesses is proportionate to the square of its velocity, and consequently the energy of the meteor exceeds that of the projectile in the proportion of the square of 40 to 1—that is, it must be as 1,600 is to 1. In other words, whatever energy the projectile may have, the meteor possesses just 1,600 times as much. It is well known that a cannon-ball or a rifle-bullet is warmed by its progress through the air; doubtless the heating process commences with the friction as the bullet passes down the tube. It is then continued by the friction in the air, and when the bullet strikes the target a part of the energy which has been preserved up to that point is then finally transformed into heat. Suppose that the temperature of the bullet is raised twenty degrees by the transformation into heat of a part of the energy which it contains, we can easily see how potent must be the heating effect on the meteor, seeing that, while only of the same weight, it contains 1,600 times as much energy as the rifle-bullet. It is quite plain that the quantity of heat generated must suffice for the exaltation of the body's temperature through a range of many thousands of degrees. We may therefore witness a transformation of the mass, not merely from the solid to the liquid form, but even further, to that streak of vapour into which the meteor is usually dissolved.

The consideration of the mechanical equivalent of heat is very instructive in the study of the Sun. Almost all the power that is displayed in exercise around us is due to the action of the Sun's beams. From the great central fire stupendous quantities of heat are shed around, and the share of that heat which finds its way to our globe is in a large degree transformed into some form of mechanical effect. Consider, for instance, that most wonderful exhibition of mechanical power to be seen at the Falls of Niagara, where it is known that many millions of horse-power are in incessant action. If we look at the source from whence the energy there put forth has been derived, it will not be hard to trace it back to the Sun. No doubt the power of Niagara is immediately derived from the overflow of Lake Erie, which itself receives the discharge from the other great lakes, fed by the numerous rivers which flow into them. But, looking a step further back, the source of supply for Niagara must of course be sought in the clouds above, from which the rain has descended, which it is the function of these rivers to collect, so that the water shall ultimately be restored to the sea. I say restored to the sea, because it is obvious that the water in the clouds has all come from vapour which has ascended from the sea. The Sunbeams, bearing mechanical energy, expend it in promoting evaporation from the surface of the water. It is the energy of Sun-heat which elevates many millions of tons of watery vapour per diem from the ocean, and bears it aloft to the clouds, from whence it descends, and is collected for the supply of Niagara. A water-wheel, turned by Niagara, virtually derives its energy from the transformation of Sunbeams.

In like manner it can be shown that the winds which blow producing, as they often do, mechanical effects on the sails of windmills or the sails of ships, derive their capacity for the performance of such work from the heat conveyed by Sunbeams. And just one instance more. We have already alluded to coal, and described it as the remains of great primeval forests. This vegetation flourished because Sunbeams shone on it, and the carbon which the coal now contains was then grasped by the growing organisms from the carbonic acid in the atmosphere. The plant is enabled to wrench the carbon from its union with oxygen by virtue of the energy with which the Sunbeams supplied it. So that when energy is to-day produced in our grates by the union of coal with oxygen, the heat which results may be said to be the precise equivalent of certain Sunbeams which arrived at this earth untold ages ago.

CHAPTER XIII.

THE SMALLEST NATURAL OBJECTS.

IN our attempt to understand the constitution of the Sun we encountered the fundamental difficulty that the conditions presented are almost totally foreign to our experience. It is not alone that the vast dimensions of the great luminary confront us with a series of problems unfamiliar to our ordinary knowledge; but the exalted temperature of the exterior part of the great glowing globe, and, so far as we can tell, the temperature of its interior also, are entirely beyond what we are accustomed to deal with in the course of metallurgical operations, or even in such experiments as can be performed in our laboratories. Experience, therefore, leaves us somewhat at fault in any attempt to interpret by reference to terrestrial analogies the phenomena that the Sun presents. We cannot raise matter to such a high temperature as that possessed by the solar materials, and consequently the behaviour of matter under such conditions as are presented in the Sun cannot be readily inferred from any observations which it is in our power to make.

We have, then, to resort to reasoning which, though founded on experimental verification, is in some degree of a theoretical character. But the process to be followed is quite in conformity with the course which has to be taken in the most practical affairs. It is one which the engineer has constantly to adopt when called upon to design something quite different from anything hitherto attempted. Suppose, for instance, an engineer proposes to span the Forth. As the scheme involves the construction of a bridge on an enormously greater scale than that of any previous structure of the kind, he is obliged to fall back on the fundamental principles of mechanics. From his studies of the properties of the materials which are available, and from his calculation of the forces to which the several parts of the erection will be submitted, he is enabled to complete the design of a bridge vastly transcending in magnitude anything before attempted. In fact, the main difficulties in the construction of the Forth Bridge had been already overcome before any part of the foundation had been laid, or before a plate of steel had been drilled. Provided that the mechanical principles by which the engineer is guided are in themselves sound, no insuperable difficulty in regard to the completion of a task arises from the fact that experience of a structure of anything like the same dimensions may be almost altogether wanting.

This illustration points out the course that we must follow in our attempts to interpret the phenomena presented by the Sun. We cannot say that the heat from the Sun is like that yielded by this furnace, or that the Sun's light is like the radiance from that electric light. We must go back to the laws of nature. Provided that we ascertain these laws correctly, and draw our inferences from them logically, we may reasonably hope to understand something of what goes on in our great luminary, even though it be wholly outside what we meet with in terrestrial experience.

To give an example of the nature of solar problems, let us consider the case of the solar materials. I speak not at this moment of the chemical nature of the Sun's elements, I refer to a still more simple matter. We are accustomed here to deal with bodies which we classify as solids, as liquids, or as gases. Now, how far may the Sun be considered as solid, or as liquid, or as gaseous? It is not solid assuredly—at least in those outer regions which alone are clearly exposed to our view; nor can there be much reason for believing that there is any part of the Sun which could be described as being of a solid character. It is unquestionable that much of the Sun must be gaseous, and in its outer regions, at all events, in a highly attenuated condition; but whether the internal parts may be partly liquid, and if so to what extent, is quite a different matter. Here the fundamental question arises as to whether in connection with the Sun we are justified in contrasting liquids with solids. One of the most remarkable discoveries in the properties of matter made in the present generation was accomplished by the late Professor Andrews, of Belfast. He demonstrated that just as a solid can be transformed by a continuous process into a liquid—just as pitch, for instance, can pass through every intermediate stage between that regarded as solid and that regarded as liquid—so there is complete continuity between the liquid and the gaseous states of matter. Such a discovery as this belongs, as Lord Kelvin well remarked, to the life-blood of science.

We think of steam as one thing and of water as a different thing, notwithstanding their chemical identity. But that liquid water should be capable of graduating by insensible degrees into gas-like steam, so that there is no recognisable point in the transformation where we can say that water ends and that steam begins, is indeed a fact which seems at variance with our ordinary experience. But this is only because the temperatures and pressures with which our daily experience makes us best acquainted are not those in which the transformation is effected in a gradual and continuous manner. The experiments of Andrews demonstrated conclusively that when the temperatures and the pressures were adjusted to points properly selected for each particular material, then the transformation from the liquid to the gaseous state was conducted by a perfectly continuous operation. The circumstances of residence on this Earth are such that matter in this extraordinary intermediate state is not met with in nature. We can only exhibit the phenomena in our laboratories.

It is, however, probable that matter in this intermediate stage, in which it is neither a liquid nor a gas, must constitute a large part of the Sun. We should be assuredly much at fault in our attempt to interpret what goes on in mighty volumes of material of this description, had we not some such guidance as is afforded by those principles of the molecular theory of matter which I am now about to consider. The vast mass of the great luminary, which is more than 300,000 times that of the Earth, introduces considerations relating to pressure wholly transcending our ordinary experience.

The recent progress in physical science has, however, been such that we are now able to refer, as it were, to the fundamental principles of the constitution of matter, and determine to some extent the actual character of the solar phenomena. Nor will it be without interest to notice that we are now about to appeal to conceptions of a very different description from those to which we are usually accustomed in astronomy. In the study of the heavenly bodies the imagination is often taxed by the effort to conceive distances the most tremendous, magnitudes the most gigantic, periods of time the most protracted. But by one of those strange surprises which so often delight the scientific investigator, the demands upon his imagination now lie in the opposite extreme. We have to consider distances so small that they elude all direct means of measurement, objects so minute that the most powerful microscope would utterly fail to reveal them, and periods of time so infinitesimal that the millionth part of a second seems well-nigh an eternity by comparison.

It is one of the great triumphs of modern science to have advanced the analysis of the constitution of matter to such a point as to enable us to study in some degree the minutest parts of which it is composed. The information thus acquired illumines with a fresh light almost every branch of physical science alike on the smallest and the grandest scale. It avails equally to trace the phenomena presented in the union of oxygen and hydrogen to form the water in a dewdrop, or to render an account of such majestic phenomena as those which have attended the evolution of the solar system. Let us begin with an outline of what is known with regard to the ultimate structure of matter. I am not now referring to chemical research, usually so-called. We know indeed by the investigations of chemists that the several terrestrial materials are composed of some sixty or seventy separate substances which are termed elements: and analysis fails to decompose an element into any simpler materials. We are also aware that many of the elements known to us on the Earth are present in the Sun, and it may be added in many of the other celestial bodies.

At present, however, our concern is not with the chemical relations of the different elements; what we rather seek is to examine the actual texture of a single one. Let us, for instance, consider that universally distributed element, iron, and study the physical properties of a fragment of this metal; we note that it is hard, tough, and heavy, and with a certain amount of metallic lustre on a freshly exposed surface. We may further suppose that the specimen we are using is chemically pure—that is to say, composed of a single element, which no process of analysis would show to be contaminated with other substances. Our examination of the iron is conducted by a simple mechanical process. We cut it up and find that each fragment, so far as we can perceive, is quite similar to the whole. Even if the material be reduced to the finest filings, each little particle seems to be completely identical in character with the original mass; as that was iron and nothing else, so the little particle is also iron and nothing else. We can suppose the sub-division to be advanced further and further until we have obtained the minutest particle that we can see. Let the process be carried on until at last the comminuted metal has been so greatly reduced that it taxes the utmost resources of the most powerful microscope to render it visible. Up to this point there is nothing to suggest that the smallest visible part of the metal possesses distinctive structure. It still seems homogeneous iron merely differing in size from the original mass. And it would almost appear as if we could go on sub-dividing without limit, and that even after the division had been carried on indefinitely, we should never arrive at anything which would be unlike the original mass of iron, except in the matter of dimensions.

In the text-books used when those who are now middle-aged were at school, the doctrine of the potential infinite divisibility of matter was taught as a fundamental truth of Nature. It certainly did seem plausible to allege that however small an object might be, it was always at least conceivable that it could be cut in half. If this were indeed an axiom of nature, then the infinite divisibility of matter would have to be regarded as a theoretical possibility. But in these latter days we have come to believe that the truth is of a widely different character from that to which the simple logic just mentioned would conduct us. It is quite true that we have never been able to handle, to form, or to see a piece of iron so minute that it could not be divided into two portions whereof each was iron still. But the intellect, trained in mathematical processes, often acquires a power of penetrating into Nature's secrets to a depth far beyond that attained by the most subtle instruments our hands can fabricate. We can reason about stars, even though they may be further off than the remotest stellar speck ever depicted on a photographic plate after hours of exposure; so, too, in the other extreme, we can reason about the sub-division of a mass of iron even when the process has been carried on to a point far beyond that to which the most potent microscope can carry our glance.

Suppose that our organs of vision were to receive an immense accession of delicacy and acuteness. Suppose that our powers of manipulation were to be increased to such a point of subtlety that we became able to handle magnitudes of an excessive minuteness much below the size of the smallest objects which can be discerned with our finest microscopic objectives. Suppose that with such powers conferred upon us, we were to resume the process of sub-dividing the piece of iron by successive partitions into halves, we should find that until an almost incredible degree of reduction had been reached, the iron particle still retained all the properties of the original mass. We know, however, that the process of bisection could not be carried on indefinitely. Sooner or later the particle of iron must have received its last possible sub-division; the little bit of metal has been separated into two parts—those parts are iron still, but the operation cannot be repeated. We do not, indeed, assert that each of the last particles is itself incapable of conceivable partition into other objects, but what we do assert is that the parts thence resulting would not possess the properties characteristic of iron.

Iron may appear to be homogeneous, but it is not really homogeneous. There is a grain in it—not discoverable, it is true, by senses like those with which we are endowed—but the existence of which modern research has rendered one of the certain doctrines of science. We find that in the last analysis by mechanical sub-division, a lump of iron is discovered to be composed of myriads of molecules, all of which can be shown to be similar in structure and in mass. The identity between the several molecules of the same substance is, indeed, one of the most astonishing facts in Nature.

But we have as yet only unfolded a very small part of the revelations of modern physics in their application to our subject. We have now to disclose an astonishing fact which would seem well-nigh incredible were it not for the irresistible logic by which it has been demonstrated. We have been conducted to the belief that a piece of iron is a congeries of uncountable myriads of molecules, and it might naturally be supposed that such an aggregation would compose a structure something like that exhibited by a block of sandstone, only that the individual molecules of the iron were much more minute than those of the grains of sand in the stone. But such a view would not give an accurate representation of the actual molecular constitution of the iron, which is, in truth, of a much more wonderful description. The several grains of sand out of whose union the sandstone is formed are in contact, being united by a cement which welds the whole into a connected mass. But the molecules of iron are not, speaking generally, in contact with each other. We must therefore think of the iron as a number of isolated points separated by intervals which are quite considerable when compared with the actual size of the molecules themselves. Here is, indeed, a radical distinction between the structure of a piece of iron and the structure of a block of sandstone. But the difference is still more striking—for it would be impossible for a collection of particles wholly separated from each other to exist if those particles remained quiescent.

Rest would be absolutely impossible in such a case. We are therefore led to the conception that the molecules of the iron must be in motion; and, this being so, see in what a different guise the constitution of a piece of iron presents itself to the eye of science from that in which it appears to the eye of an ordinary observer. Instead of the homogeneous solid metallic mass which is displayed to our ordinary vision, we are to think of the iron merely as a stupendous multitude of little molecules in rapid vibration.

So strange an assertion with regard to the ultimate constitution of matter naturally challenges our closest scrutiny. I cannot attempt, in these pages, to enter into any full discussion of the demonstration by which it has been proved that molecules of iron are really in a state of rapid movement. But I will give an illustration which may serve, at all events, to show the line of reasoning employed.

Let us suppose that we are listening to a performer on the violin. The sounds are actually conveyed to the ear by undulations through the atmosphere; and as the pulses of the air strike on the drum of the ear, they set that drum in vibration, and our brain translates the vibrations into the music which so much delights us. The necessary agitation of the drum of the ear can only be produced by the expenditure of work done by the arm of the performer as he draws his bow across the strings of the instrument. The air is thus to be regarded as the vehicle by which energy has been conveyed from the arm of the performer to the ears of his audience, and the transmission of the energy is effected by the aerial undulations which constitute sound. But it will be obvious that air can only be made to undulate by a material agent which is itself in motion, and in this case we find the source of the aerial movement to be the sounding-board of the violin. That sounding-board is in active vibration, the movement being communicated to it by the string, which is itself agitated by the bow. If the vibrations of the sounding-board cease, then of course the atmospheric undulations cease; as the vibrations of the sounding-board wax or wane in amplitude, so the undulations transmitted by the air become of greater or of less intensity. This illustration will serve to show that undulations propagated through a medium must originate in the vibration of some material by which the medium is directly affected. Let us therefore apply reasoning of a similar description to the interpretation of certain analogous phenomena in a wholly different case.

Sound, as we have said already, is transmitted by means of undulations in the air, but at present it is another medium which concerns us. Space in the solar system, and throughout the inter-stellar regions, is filled by a medium almost infinitely more subtle than air of the most rarefied description. It is that ether which not only pervades such spaces as we should ordinarily describe as being vacuous, but which also penetrates through and through the atmosphere itself, and passes through and through the texture of solid bodies—cast steel is as permeable to ether as a grove of trees is permeable to wind.

Ether transmits undulations, just as undulations are transmitted by air; although it is true that in many respects the character of the ethereal waves is widely different from that of the atmospheric waves. In the atmosphere we know that the propagation of a wave is effected by each particle of air moving to and fro in a line which coincides in direction with that in which the whole wave is advancing. But in the case of ethereal undulations, it would seem that the vibrations take place perpendicularly to the direction in which the wave is moving. In this respect at least we may liken the ethereal waves to those more familiar ones on the surface of the sea; the wave as a whole advances, but each particle of water only moves up and down. A cork, for instance, when floating on the water while the wave passes by, moves up and down, but has no movement along the surface comparable with that by which the wave itself is animated.

It is not necessary for us to enter into detail with regard to ethereal waves. Suffice it to say that such waves are produced, and that they are of the utmost importance in nature. It is the waves of ether which convey the sensations of light to the eye much in the same way as the waves of air convey sound to the ear. Sounds could not traverse the space intervening between the Earth and the Sun, because it is devoid of air, by which alone the undulations of sounds can be propagated. As, however, space is full of ether, the vibrations of light can be transmitted, and the character of the light depends on the lengths of the waves. We are not at present concerned, however, with the ethereal waves which affect our sense of vision. What we have now to deal with is the way in which these ethereal waves affect our sense of feeling. Certain waves acting on the organs of vision give us the perception of light; waves of quite a similar description, though differing it may be somewhat in their period, give us, when acting on our nerves of feeling, a perception of heat. Provided with this conception, we are now enabled to demonstrate the important fact that the molecules in iron are in rapid movement.

For let us take the case of an iron casting which is still red hot. As we stand a few feet from it we feel the warmth which it dispenses so liberally; this warmth is, as we have seen, generated by the movement of waves which cross the intervening space. These waves impinge on our nerves, and, by the vibrations which they impart to the nerve fibres, convey to us impressions which the brain translates into the sensation of heat. Have we not here a case somewhat analogous to that of the violin, to which we have already referred? We find in each case our organs of sense to be affected by the incidence of undulations upon the nerves with which those organs are provided. It is true that the vehicle by which the undulations are carried is not the same in the two cases; in one it is the air, in the other it is the ether. For our present purpose, nevertheless, both arguments follow precisely the same lines. Energy has to be expended in each case in the production of the appropriate sensation, and that energy is transmitted through the vibratory medium in which the waves are propagated. Just as we traced the vibrations of the air up to the sounding-board of the violin, and found that the sounding-board must be in oscillation—for otherwise the air would not be set in motion—so now we can follow in imagination the ethereal waves from where they strike our nerves of feeling up to the hot iron from which it is plain that they originate.

The energy with which the ether is agitated must have come from the iron, and the analogy of the violin shows us that since the ether is made to undulate, there must be some material motion in the iron mass. But yet, as a whole, it is clearly at rest; even our microscopes would fail to indicate that the smallest part of it was moving. Movement, however, there must be, and following this line of argument we can demonstrate that the movement is to be sought in the ultimate particles of which we have seen the iron to be composed. Here, then, we learn that in the case of a red-hot body each molecule is in a state of rapid vibration, these vibrations being so communicated to the ether as to set it undulating, and thus start in it a series of waves which produce the sensation of heat whenever they fall on properly recipient nerves.

But it may be objected to this demonstration that though it suffices to prove that a piece of red-hot iron has its molecules in rapid movement, it does not demonstrate that a mass of cold iron must be similarly affected. It is quite true that the molecules in a cold body do not move so rapidly as do those of the same body when heated, but it will not be hard to demonstrate that even in the case of a lump of iron at an ordinary temperature, rapid molecular movement is a necessity. For suppose that a lump of iron at any temperature were placed in a chamber, the walls of which were maintained at a lower temperature than the iron, the temperature of the iron would begin to fall—that is to say, its heat would be radiated away, and, so far as it went, would be available for the exaltation of the temperature of the surrounding cooler objects. Here, then, we see that a piece of iron even at any ordinary temperature, such as that of the air, does under certain circumstances radiate heat. But we have already shown that the radiation of heat necessitates vibrations in the molecules of the radiating body; it follows that a piece of iron at any temperature—save, perhaps, at the absolute zero itself—must have its myriad molecules in rapid vibration. This is, indeed, a surprising result; but it is one to which we are led by so many independent lines of research that it seems impossible to admit even a doubt on the matter.

What I have said with regard to iron may be applied to all other solid bodies. We are to consider every mass of matter as composed of invisible molecules, so long at least as the composition of the body can be described in the same chemical terms. Each of these molecules is, generally speaking, separated from its neighbours by intervals which are wide in comparison with the dimensions of the molecule itself; and each molecule is animated by rapid vibration. The hotter the body the greater is the speed with which its molecules swing; in fact, we may generally assert that the heat of the body is proportional to the square of the speed with which its molecules are on the average animated.

The molecular theory of the constitution of bodies will enable us to form a somewhat clearer notion of the real physical distinction between the solid, the liquid, and the gaseous states of matter than would have been possible on a less correct hypothesis. In a solid it would appear that, notwithstanding the rapid movements executed by each molecule, its excursions are necessarily restricted within certain limits. In fact, its wanderings are confined to the immediate neighbourhood of the particles with which it is closely associated, so that it has no opportunity of extending its journeys into remote parts of the mass. But when a body is sufficiently heated it passes from the solid into the liquid state; in this condition the movements of the molecules are performed more rapidly, and it is evident that the amplitude of the excursions which each molecule is permitted to make has become much wider than when the body was solid. It is no longer compelled to remain always in association with the same immediate neighbours. It seems as if a molecule could transfer itself from one group of neighbours to the next, though still far from possessing what might be described as perfect freedom. In this way it is possible for one molecule to travel, in course of time, to a considerable distance from its original position.

Simple experiments will demonstrate this. Suppose there be two portions of water alike in all respects save that one of them is coloured with some infusion which will enable its movements to be traced through another. Let these two liquids be placed one above the other in the same vessel. It is found after a time that these two liquids begin to mix, as would be ordinarily said, or to diffuse one into the other, as the natural philosopher would say. In the course of time portions of the coloured liquid will be found to pervade the whole of the uncoloured portion. The only explanation of this phenomenon is that some of the molecules in the coloured part have freed themselves from their immediate neighbours, have crossed the boundary between the two liquids, and have gradually penetrated to a long distance from where they were originally placed. This in itself is a demonstration of the mobility which the molecules of a liquid enjoy.

But we have another step to take in order to comprehend the transition from the liquid form of matter to the gaseous. It would appear that even in the most mobile liquid the molecules can never be described as being at any moment wholly free from their neighbours; they shake clear from one surrounding group merely to ally themselves with another. But when by the application of still further heat the liquid is transformed into a gas, then much wider latitude is permitted to their wanderings. The influence of contiguous molecules on the movements of any individual molecule, instead of being incessant, as in the case of solids, and in a minor degree of liquids, is now exceptional and intermittent.

The molecules of a gas enjoy a liberty which entitles them to be called perfectly free bodies during the greater part of the time. Imagine the existence of multitudes of minute elastic particles, moving quite independently, with extremely rapid velocities. If there be a very large number of these molecules in an extremely small portion of space, then there will be occasional encounters between them. They need hardly be described as actually striking one against the other in such encounters, but when in the course of their rapid movements two of these molecules make a close approach, there is intense mutual action, just as we sometimes find in the case of two stars, when in the course of their journeys through space it happens that they come close together, even though they may not, strictly speaking, collide. Each of the molecules during such an encounter is deflected from its course; however, it speedily passes beyond the distance within which the mutual action is appreciable, and then the little body makes another free run until it experiences an encounter with another molecule belonging to the vast swarm of which the gas is composed.

We thus see that each little molecule moves in a zigzag track, with an ever-varying speed. Every turning-point in its career is one in which the chapter of accidents has brought the molecule into contact of a more or less perfect description with some other similar molecule. The more dense the gas the more numerous must be the molecules within a given volume, and therefore the more frequent will be the encounters which each individual molecule experiences. In highly rarefied gas, like that contained in one of Mr. Crookes's radiometers, in which a high vacuum has to be kept up, there are so few molecules present that each individual makes a comparatively long run between two successive encounters. If the quantity of heat in the gas be increased, then the average speed of the molecules will likewise be increased, and thus we are led to the important notion that the temperature of the gas depends upon the speed with which its molecules are dashing about.

So startling a view of the composition of air and of every other gas known to us certainly seems to demand some very clear demonstration before we admit it to represent the facts of Nature. Let us therefore consider how this doctrine may be made to render account of the most fundamental properties of matter in the gaseous state. Take, for instance, that elastic property by which gas is said to press equally in all directions. A cubic inch of air at the surface of the Earth produces, as we know, a pressure of about 15 lbs. on each of the faces of the cube which bounds it. This pressure is exerted upwards as well as downwards, to the right as well as to the left. Now how are we to explain it? Of course, we may say that it is due to the elasticity of the gas, but we must be careful not to be satisfied with a mere form of words where physical interpretation is what is really wanted. The molecular theory of the constitution of gases provides the requisite interpretation. We have seen that a gas is composed of myriads of molecules, each animated with a velocity of which the average value has been determined, although it varies enormously in different molecules.

We do not attempt to follow the fortunes of any individual molecule in our calculations. We can only deal with them collectively or statistically, as the process is called. It is plain that the several molecules, when confined within definite boundaries, must occasionally rap against the walls of their chamber. Although these raps are excessively insignificant individually, yet in their myriads they produce the actual pressure, and as the molecules are moving up as well as down, to the right as well as to the left, with speeds which are on the average identical in every direction, we see that the hammering which the ceiling of a little cubical chamber will receive is almost the same as that which will be delivered on its floor, or as that which will be transmitted to each of the walls.

Thus we explain the fact that a gas presses equally in all directions. If the gas be compressed we can now understand how its pressure is found to increase according to Boyle's well-known law. For suppose that the cubical chamber of gas which measures one inch each way undergoes a diminution of volume to half an inch each way, then the molecules will be compelled to restrict their movements within a space which is only one-eighth of that through which they were originally permitted to roam. The several molecules will consequently be on an average eight times more compact, and therefore the number of raps that would be delivered on a given area would be eight times as numerous as on the same area before the compression took place. If we suppose the temperature to be restored to what it was at first, the speed of each molecule, and therefore the vehemence with which each rap is administered, will be on the whole the same in the gas as it was originally and after the compression has taken place. Thus we see that the pressure per unit of area will be increased eightfold by compression. But this is in exact accordance with Boyle's law, which asserts that with unaltered temperatures the pressure of a given quantity of gas varies inversely as its volume.

As another illustration of the molecular properties of gases, let us suppose that the original cubic inch of gas, while remaining unaltered in dimensions, has had its temperature raised. Then the theory declares that the average pace with which each molecule moves is increased. It therefore follows that the vehemence with which each molecule raps against its enclosing wall must be correspondingly enhanced. Thus the mere fact that the temperature of the gas has been raised increases the pressure which it brings to bear on the sides of the surrounding chamber.

The constitution of gases has been studied so diligently in the light of the molecular theory that we have learned something about the actual numerical quantities involved. It is obvious that some information as to the average pace with which the molecules are moving would be of fundamental importance. Let us take, for instance, hydrogen gas. We know the density of this element, and we can calculate what the speed must be with which, on the average, the molecules rap against the environment of their chamber so as to produce the pressure which observation shows to be actually experienced. It is thus discovered that the molecules of hydrogen, when at the freezing temperature and at the ordinary barometric pressure, move on the average about sixty-nine miles per minute. Of course it will be understood that many molecules move with a speed much slower than this, and many others much more rapidly. It has been already said that we are never able by any device of calculation to follow the individual fortunes of any particular molecule. It must therefore be understood that the result at which we have arrived is only an average one.

The less the density of any gas, the higher is the velocity with which its molecules move under the same conditions of temperature and pressure. For example, as hydrogen is so light, each molecule will have to deliver its rap at a much higher speed than would suffice to produce the same effect with a heavier molecule. It is thus found that the average pace with which a molecule of oxygen or of common air is animated, is about eighteen miles a minute under the same circumstances to which the molecules of hydrogen correspond with a speed of sixty-nine miles a minute. It may serve to impress this important figure on the memory to remember that while the pace with which the Earth moves in its orbit round the Sun is about eighteen miles per second, the speed with which the molecules of our atmosphere are ordinarily flying about averages the same number of miles per minute.

These numerical facts are important in connection with astronomical investigations, and it is therefore fortunate that they happen to be ascertainable with considerable accuracy. There are, however, some other quantitative elements connected with the theory of gases of which estimates can be arrived at. Certain experiments afford the means of determining the average length of the free path which a molecule of a gas pursues in the interval between two successive encounters. For example, in the case of hydrogen at the freezing temperature, and under the ordinary pressure, this distance appears to be about the ten-thousandth part of a millimetre. The conception of this very small magnitude may perhaps be better understood if I say that more than a thousand of such distances would be comprised within the thickness of this sheet of paper.

We have also some knowledge of the dimensions of a molecule. Four or five processes have been suggested by different philosophers, which point out what the diameter of a molecule appears to be. Professor Clerk Maxwell states that two millions of molecules of hydrogen in a row would occupy a millimetre. Lord Kelvin has given an illustration of the presumable size of molecules in the following striking manner:—Let us imagine a raindrop the size of a pea to be magnified into a globe as big as the Earth. If every molecule of the water was increased in its proper proportion, then the result to which we are led is that the sizes of the several molecules would be larger than grains of shot, and smaller than cricket balls. It is also possible to form a notion of the weight of a molecule of hydrogen, and thus we find that two hundred million million million of them would weigh a milligramme; while the multitude in which the molecules are present is such, that the smallest particle visible under the microscope would, it has been calculated, hold from sixty to one hundred million molecules of gas.

The picture overleaf (Fig. 67) exhibits the mare crisium on the Moon enlarged from a photograph taken at the Lick Observatory. I give it here in illustration of a remarkable phenomenon connected with our satellite. The clearness of the picture is due to the fact that the Moon is not surrounded by an atmosphere. Indeed the absence of such a covering to the Moon is one of the most striking facts connected with our system. It can be shown that this lunar destitution is completely explicable by the supposition that air has the molecular structure we have described in this chapter. The small mass of the Moon has rendered it incapable of attracting to itself a sufficient multitude of those rapidly moving atoms which would be required to compose an adequate envelope.

FIG. 67.—THE MARE CRISIUM ON THE MOON.

(From a photograph at the Lick Observatory.)

As to the actual physical character of the molecule itself, we are at present but little informed. It seems certain that, notwithstanding its excessive minuteness, it is so far from being a mere point, or simple centre of force, that it must be regarded as a veritable structure possessing no little complexity. Besides that movement of translation which we have already referred to, the molecule is also endowed with a rotation on its axis, and the several parts of which it is constituted have relative movements, which seem, so far as we can tell, to be of extreme complexity. There is good reason for believing that the lines in the spectra of incandescent gases must be attributed to displacements occurring inside the molecule itself. From our knowledge of the wave-lengths of these lines it becomes possible to learn something of the rapidity with which these intra-molecular oscillations are effected. Here, indeed, we come to considerations which tax the intellect just as greatly in one direction, as the conception of star distances do in the other. Notwithstanding the excessive minuteness of the molecule, it follows, from certain spectroscopic phenomena, that oscillations must take place within it at the stupendous rate of many billions per second.

Professor Langley tells us of a dynamite explosion which he heard. Three waggon-loads of the material accidentally ignited at a distance of two miles from where he was standing. The extraordinary suddenness of the noise arrested his attention. He visited the locality; every trace of the waggons had disappeared, and a vast hole in the ground appeared where they had stood. The whole phenomenon by which such terrific havoc had been wrought apparently occupied but a fraction of a second. But suppose a being who lived in a world where such oscillations as those performed in the molecule of hydrogen corresponded to the units of time that enter into ordinary conception. To a being who could count at appreciable intervals the successive oscillations of a molecule of hydrogen, that dynamite explosion would have seemed a more tedious operation than the formation of this Earth, from the earliest deposition of palæozoic rocks up to the present moment, does to us.

CHAPTER XIV.

THE HEAT OF THE SUN.

ONE of the most important problems which we have to consider in this volume is connected with the method by which the Sun's heat is sustained. The torrents in which that heat is poured forth every hour are sufficiently astonishing, and when we reflect that Sun-heat has been dispensed for periods to be reckoned probably by millions of years, the question as to the supply of a radiation so prodigious assumes a very striking form. As to the actual temperature which the Sun possesses, we must admit at once that considerable uncertainty still prevails. We do not know the temperature which the luminary would show if we were actually able to plunge a thermometer into it; but among the various estimates which have been made, perhaps that most entitled to our confidence is by M. H. Le Chatelier, who made a contribution on the subject to the French Academy on March 28th, 1892. He performed a number of experiments on the intensity of the radiation emitted by an incandescent body. The temperatures employed ranged from 680° to 1,770° centigrade; and from the results of his measurements he was able to deduce a minor limit to the quantity under consideration.

It would seem from his experiments that the temperature of the Sun is about 7,600° C.—that is to say, a body as large as the Sun raised to the point named, and placed at the same distance as the Sun, would transmit to us exactly as much heat as we actually receive. Chatelier points out, however, that the real temperature of the photosphere must be much higher than this, inasmuch as a considerable amount of the heat which it radiates is absorbed by the cool atmosphere surrounding the Sun, and we only receive the balance which escapes; all that can be certainly said is that the temperature of the Sun seems

PLATE IX.

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SOLAR PROMINENCES.

much higher than the greatest which can be produced in any of our furnaces.

But there is another way in which the question of the Sun's heat may be numerically estimated. For this we employ the calorie as a unit of heat. It signifies the amount of heat needed to raise, by one degree centigrade, the temperature of a kilogram of water. Consider a square metre on the surface of the earth, then upon that square metre a definite quantity of heat is poured every minute. This amount is obviously an important constant expressive of the effectiveness of the Sun as a source of radiant heat. Professor Young, in his well-known work, estimates its value—apart from the effects of atmospheric absorption—at twenty-five calories per square metre per minute. But he remarks subsequently that in consequence of Professor Langley's investigations it has become necessary to assign to the constant a somewhat higher value, and accordingly we now adopt for the Solar Constant about thirty calories per square metre per minute.

This will, perhaps, be best understood by conceptions of a different character. In order that thirty calories should reach the Earth, 46,000 times thirty calories must leave the Sun, the number 46,000 representing the proportion between the squares of the distances of the terrestrial and solar surfaces from the Sun's centre. Let us now suppose that a single square metre of the solar surface were employed beneath a boiler for the purpose of generating steam; then 1,380,000 (46,000 × 30) calories per minute would pass into the boiler, and the generation of steam that would result would be capable of developing, according to Lord Kelvin's estimate, not less than 78,000 horsepower. Indeed, Professor Young's figures would give a result even higher. In other words, each square metre of the Sun's surface pours out enough heat to maintain about half a dozen mighty Atlantic steamers at their utmost speed night and day, from year's end to year's end. This gives some idea of the profusion with which the solar heat is lavished.

The efficiency at which the Sun is now working is given by computing the number of square metres on its surface; and then, supposing that each is capable of yielding 78,000 horse-power, the total thus obtained is not less than four hundred and seventy-six thousand million million million. We know for certain that this mighty expenditure has been going on for unnumbered thousands of years, and it becomes a question how the necessary supply of energy can be maintained.

At first it might be imagined that the Sun was a highly-heated solid sphere, gradually dispensing its heat by radiation. It can, however, be very readily shown that such a supposition would not explain the known facts of solar history. Were the Sun a solid, its outer layers would have parted with so much of their heat within a few years that the luminary must have greatly declined in temperature. The heat to maintain the perennial outgoing could therefore only have been sustained by supplies transmitted from within outwards. Such a mode of supply would, however, demand for the solar materials a capacity for conducting heat vastly in excess of that possessed by any known bodies. It is then perfectly certain that the Sun could not, if it were a solid, sustain its present radiation for even a small fraction of the time that we know it has actually radiated without perceptible abatement. We are therefore forced to the conclusion that the Sun cannot be a solid body.

Other considerations developed in this book confirm the present demonstration. The Sun is, therefore, a fluid of some description. Undoubtedly it is gaseous on the outside, and it probably passes, by insensible transitions in the interior, through those intermediate states of matter which Professor Andrews's discoveries have revealed to us, to a central portion of dense liquid. If this be the constitution of the Sun, our first great difficulty as to the supply of its radiation is removed. We know that when water is boiling on the fire the heated parts from the bottom of the vessel ascend through the mass, and thus quickly convey the heat to those parts of the vessel where heat is being lost by radiation, or is required in the process of evaporation. In a somewhat similar way, there can be no doubt that as the intensely heated materials in the photosphere lose heat by radiation and become comparatively chilled, they sink down towards the Sun's interior. They are then replaced by fresh masses of hot gas from beneath, which, on coming to the surface, continue the radiation, until, in their turn, they undergo a loss of heat which causes them to sink down, when they are again replaced by fresh glowing matter. Thus we explain the flow of heat from the Sun's interior to its surface.

But it must not be supposed that this disposes of the primary difficulty as to how there can be enough heat in the Sun to maintain the current annual expenditure. The actual heat that is at present in the Sun's interior would not supply its radiation very long. We must therefore see if there be not some way in which its amount can be augmented by the transformation of something else into heat. It can be demonstrated that if the Sun were a solid lump of coal from surface to centre, and if that lump of coal were supplied with oxygen abundantly sufficient for its combustion, it would not be sufficient to supply the solar radiation for more than a few thousand years. Here is, indeed, an enigma. For, considering the enormous quantity of heat accompanying the combustion of coal, does it not seem strange that the Sun's heat should really transcend that produced by the mighty conflagration I have suggested? But such is indeed the case. Professor Langley well remarks that though there is coal enough in the State of Pennsylvania to supply the wants of the United States for many centuries to come, yet the heat which could be generated by the combustion of all the coal in Pennsylvania would not be sufficient to supply the Sun's radiation for the thousandth part of a single second.

It has been thought that the solar heat could be recuperated by contributions from outside. It has, for instance, been urged that the falling of meteors into the Sun generates an amount of heat which must go far to compensate for the Sun's actual expenditure. There can be no doubt that some heat might be, indeed must be, derived from this source. We know that the flash of a meteor in our atmosphere is attended by the evolution of heat. The heat is, indeed, generally sufficient to warm the body by friction against the air, and to render it red hot, white hot, and even dissipate it into vapour. There can be no doubt that meteors do indeed dash into the Sun far more frequently than they do upon the Earth. It is certain that as each meteor plunges into the great luminary it rushes to its doom with a speed which must be on the average far greater than that possessed by any meteor projected through our atmosphere. There will, therefore, be a proportionate increase in the quantity of heat produced by a meteoric shower on the Sun as compared with that generated by a meteoric shower on the Earth. But it can be readily shown that no effective contribution to the maintenance of the solar radiation need be expected from such a source. It has been demonstrated that a quantity of meteoric matter, with a collective mass about equal to that of the Moon, would, if it rained into the Sun, suffice to supply the solar radiation for a twelvemonth. But there are the best reasons for believing that the compass of the solar system could not provide anything like the continuous supply of meteoric matter which this view would demand. Not one-thousandth part of the solar radiation could be accounted for by the actual influx of meteors; indeed, probably not one-millionth part.

So far we have only established the negative conclusions that the heat of the Sun can neither be attributed to the mere cooling of a great heated body, nor can it be sustained by anything like combustion. Nor yet, again, have we found that the meteoric theory as to the source of Sun-heat has any reasonable claim to our assent. The true theory on the subject is that which was advanced by Helmholtz. It is founded on certain dynamical principles certainly known to be true. The explanation is perhaps a little difficult, but it must, nevertheless, be attempted, inasmuch as it involves the most vital doctrine on the relations of heat to the solar system.

The condition of the Sun as to temperature and as to gravitation are so widely different from those which we find on the Earth, that we are compelled to fall back on fundamental principles to interpret the phenomena which these conditions involve. It is, indeed, fortunate that we have the sure and steady light of dynamics to guide us in an investigation which would otherwise be of a necessarily insecure nature. Here, too, we have to employ that theory of the constitution of matter which has been developed in the last chapter. We must, then, think of the photosphere as composed of myriads of molecules, each of which is moving, as the molecules of a gas are wont to do, in what is practically a straight line between one encounter and the next. With what a portentous row of figures we should express the actual number of molecules which exist in the Sun it is not now necessary to concern ourselves; suffice it to say that from surface to centre every part of the Sun is composed of molecules, each of which moves in accordance with well-known dynamical laws. It is true we may not be able to pursue the intricacies of each individual molecular movement, but that is, happily, not necessary. We can determine by the statistical method of reasoning the average result as regards all the molecules, and that is all which need concern us.

We have shown that the temperature of a gas depends upon the speed with which its molecules are animated. It is no doubt true that a part of the energy possessed by a gas exists in the form of intra-molecular vibrations. But this must be connected with the energy due to molecular translations. For it is obvious that the more rapid the motions, the more violent will be the encounters, and the greater the intra-molecular vibrations which these encounters sustain. We may for our present purpose regard the quantity of energy contained in the molecule itself as proportional to the energy possessed by the molecule in virtue of its movement of translation. If the gas be parting with its heat, the velocity with which the molecules move becomes correspondingly lessened. As the Sun loses heat by radiation, the velocities with which its molecules are animated would tend to decline in consequence of that loss of heat. Unless there be some agent tending on the whole to increase those velocities, and thus counteract the loss that they experience in consequence of radiation, it is inevitable that the efficiency of the Sun as a source of heat must diminish. But we have already stated that, so far as observation goes, there is no indication of any such decline in the radiation.

It is therefore perfectly certain that the lessening of velocity which the molecules are threatened with in one way, must be neutralised by some antagonistic action. On the whole, the velocities of the molecules, or, to speak more correctly, the average squares of the velocities throughout the Sun's mass, must remain unaffected, or at least can only vary within limits narrower than those to which our observations extend. But on physical grounds it is perfectly plain that there is a force constantly in action on the Sun which, generally speaking, tends to increase the velocities with which the molecules move. That force is the gravitation of the Sun itself. On the Earth a body tends to fall in consequence of terrestrial gravitation, and the rate at which it acquires velocity vertically downwards is about thirty-two feet per second. Owing to the vastly greater mass of the Sun the attraction with which it draws a body towards it at the Sun's surface amounts to more than twenty-seven times as much as terrestrial gravitation at the surface of the Earth. It follows that in each second the Sun will impart to any free particle near its surface a velocity of about 864 feet per second, directed towards the Sun itself.

According to the Second Law of Motion, this velocity will be imparted quite independently of any velocity with which the body may have been previously animated. If, for example, a missile were shot upwards from the Sun, then the velocity with which it started would be gradually reduced at the rate of 864 feet per second, until the upward velocity had been entirely destroyed; the body would then come to rest for a moment as it turned to commence its descent. If, on the other hand, the body had been projected vertically downwards towards the Sun, from a point a little above the surface, then an addition would be made to its initial velocity at the uniform rate of 864 feet per second. Finally, let us suppose a body projected tangentially over the Sun's surface like a shot fired from a cannon, the influence of the solar attraction will bend the missile downwards, and at the end of a second, while retaining its horizontal velocity unabated, it will also have acquired a velocity downwards at the rate of 864 feet a second.

These principles must apply equally whatever may be the dimensions of the bodies on which the solar attraction operates. They may be as large as cannon-balls, or as grains of sand, or as molecules of matter; and it is in the application to these last of the gravitational principle that we shall find an explanation of the source of the Sun's heat. Incalculable myriads of particles are dashing about in the photosphere with the enormously high velocities appropriate to the exalted character of its temperature. Whatever may be the movement by which each of these little molecules is animated, whether it may happen to be directed upward or downward, to the right or to the left; no matter what be its collisions or encounters with other molecules, the gravitation of the solar mass will invariably tend to add a component of downward velocity amounting to 864 feet per second.

It is true that the effect may not be to bring the molecule nearer to the Sun's surface, but it is certain that in every case the molecule will always, at the end of each second, have a velocity of 864 feet towards the Sun's centre more than it would have had if the Sun's attraction had not been present. A little consideration will show that this influence must, on the whole, tend to increase the velocities of the molecules. If, for instance, we suppose them all at rest, then it is at once obvious that, starting without any velocity at all, they will, by the end of the first second, have each acquired a velocity of 864 feet per second. It is also plain that if they were moving horizontally, then, while that component of their pace remained unaffected, there must be a distinct addition of a vertical component, the joint result being, of course, to increase their speed. No doubt a molecule whose original motion was vertically upward would lose velocity by the action of gravitation, and its net velocity being therefore abated, there would be to this extent a decline in temperature. On the other hand, if a molecule were moving directly towards the Sun, the entire effect of gravitation would be expended in increasing its velocity, and it would therefore show an increase of temperature which would more than compensate for the loss experienced by any molecule which happened to be travelling exactly in the opposite direction. We might expect, as the outcome of gravitation on the whole system, that the squares of the velocities would be augmented.

Thus we see that gravitation to the Sun would steadily tend in the direction of increasing the mean square of the velocities of the molecules, while the effect of radiation and the losses that it entails would be to lessen the temperature, and therefore the mean square of the same velocities. If one of these influences suffices to counterbalance the other, then the difficulty as to the sustenance of the Sun's heat is sufficiently explained. This is a question which has to be decided by exact numerical calculation. It has been found that the gain of energy by the molecules through gravitation towards the Sun compensates for their losses in virtue of radiation. Thus the difficulty about the sustenance of the Sun's heat has been removed.

But we can imagine that the following difficulty may present itself to the mind of the reader. Take the case of the Earth's atmosphere, then every molecule which it contains is, of course, attracted by the Earth, and must, on the whole, acquire a velocity of thirty-two feet per second towards the Earth's centre over and above what it would have if the Earth's gravitation were annihilated. How, then, does it come about that gravitation does not produce thermal phenomena in the Earth's atmosphere analogous to those which it produces in the solar atmosphere?

The difference is thus to be accounted for. The Earth having already become hard and rigid in its exterior portions no longer experiences a progressive reduction of bulk to any considerable amount. No doubt so long as internal heat still remains in the central parts of our globe, heat must gradually leak out to the surface, and thence be radiated away into space. As the consequence of this loss of heat, it follows that the Earth must be contracting to some minute extent; but the leakage of heat is so slow that the actual shrinkage in the diameter of the Earth is quite too small to be perceptible. The case is thus altogether different from the effects produced by the thermal expenditure of the Sun. In consequence of the excessive fervour of the solar photosphere, the loss of heat thence is prodigious, and consequently the contraction arising from that loss of heat must be much more considerable than it would be if the Sun radiated no more heat than a cold body like the Earth. It will further be observed that a gaseous body such as the Sun must necessarily, according to the laws of heat, contract more than a solid body would do for the same fall of temperature. It therefore follows that we need not look for any parallel to what goes on in the solar atmosphere in the actual circumstances of the terrestrial atmosphere, in so far as the process of cooling is concerned.

The particles in our air, though they are no doubt attracted by the Earth, do not, on the whole, approach the centre of the Earth to any appreciable extent. In the language of mechanics, the gravitation of our globe does not now do any work on the particles of our atmosphere. Gravitation cannot, therefore, on the whole, augment the velocities of atmospheric molecules, and accordingly the gravitation of the Earth does not affect our atmosphere in the same way as the solar atmosphere is affected by the mighty body which it surrounds.

In this subject it is very important to remember clearly the distinction between force and energy. The mere application of force does not necessarily imply that the molecules acted upon must necessarily gain heat or acquire energy in any other form. The molecules of the Earth's atmosphere are of course acted upon by the gravitation of the Earth. It is true the intensity of terrestrial gravitation is very much less than that exerted by the Sun, being in fact only one twenty-seventh part of its amount. The essential difference between the two cases is that on account of the shrinking of the Sun the attraction of its mass actually performs work upon the atmospheric molecules. It communicates energy to them which, if not shown by an increase of their velocity at least suffices to supply their loss of heat by radiation.

At first it might appear that the utmost amount of heat which could possibly be generated by the shrinkage of the Sun could never equal the stupendous amount which is perennially dispensed from it by radiation. This part of the question admits of strict calculation. It can be shown that if gravitation were sufficiently effective on the particles of the solar mass to reduce the diameter of the globe by about 220 feet annually, then the heat which would be generated in consequence of the acceleration of the average velocity of the molecules would be quite sufficient to provide for all the losses involved in a year's radiation of Sun-heat. So trifling a diminution in the vast size of the Sun could not be made perceptible by our measurements. The actual alteration in the solar diameter to which this contraction would lead, even after a thousand years, would be too minute to be accurately determined. It must be remembered that in this view of the source from which the Sun's heat is sustained we are introducing no mere hypothetical agency. It is a certain consequence of the laws of dynamics that the Sun's gravitation must produce an acceleration of molecules in the solar atmosphere. Nor is there any room for fallacy in the calculations by which we ascertain the quantity of heat to which such movements correspond. There is therefore no doubt whatever that the theory of Helmholtz gives the true explanation of the great problem of the source of Sun-heat.

There is another point whence to view the same theory, which may, perhaps, present fewer difficulties to some readers than the process we have already indicated. It depends on certain simple dynamical principles. It is obvious that heat can be generated by employing the falling of a weight as a source of power. We may, for instance, set a piece of mechanism in motion by the descent of a mass of iron suspended from a rope passing over a pulley, and arrange it to work rubbing surfaces so that heat shall be generated by friction. We might again drive a dynamo by a contrivance actuated by falling weights, and the electricity which that dynamo generated might be transformed into heat. In fact, as we have explained in chapter XII., it has been demonstrated by the Mechanical Theory of Heat that the descent of a weight through a certain distance, representing as it does a certain quantity of work or energy, is equivalent to a definite quantity of heat.

The heat produced bears a simple relation both to the weight and to the distance through which the weight descends. It is proportional to their product. For example, if the weight fall through 100 feet, a certain quantity of work done is equivalent to a certain amount of heat. If the same weight descend through 200 feet, then twice the quantity of work is done, or twice the quantity of heat is produced. If the distance through which the weight descended remained the same, while the weight itself was doubled, then double the quantity of heat could be generated by the mechanism to whose movement the power of that weight was applied. We can employ this principle to explain the generation of heat by the contraction of the Sun. It is true that the weights which we are now to consider are not solid metallic masses; they are rather mighty volumes of glowing vapour. But it must be observed that this makes no real difference in the principles involved. The outer layers of gas on the Sun are gradually drawing in towards its interior. They may therefore be regarded as falling weights, and the amount of heat which their descent could generate is quite determinable. It should also be observed that we know from the laws of Nature that energy is never lost. Whenever a weight descends through a certain distance, work of some kind must be done, and hence we are assured that the energy, which appears to be produced by the descent of the solar materials towards the Sun's centre cannot be lost; it only undergoes transformation, and the form into which it becomes transformed is unquestionably heat.

Nor will it be difficult to show that the heat which could be generated in this manner may be quite commensurate even with the vast quantity necessary to sustain the Sun's daily radiation. A few figures will perhaps convince us of this. Let us take the element hydrogen, which we know to be very abundant near the Sun's photosphere. This gas is the lightest body in Nature; but light though it be, such volumes of hydrogen as the Sun must contain, possess a weight that is not easily realised. A cubic mile of hydrogen on the Earth's surface would weigh about 367,000 tons. What a cubic mile of hydrogen weighs on the surface of the Sun it is not, perhaps, quite easy to say. If the temperature and pressure were the same as those just assumed, then we know from the principles already explained that the gravitation of the Sun would make the weight of the cubic mile of hydrogen twenty-seven times as great as that of the same quantity here. But of course the conditions actually present in the photosphere differ widely from those which prevail on the Earth. The great exaltation in the temperature of the photosphere would make the gas much lighter if the pressure were the same. On the other hand, the photosphere is charged with vapours of metals and other materials which must add to its weight; so that a cubic mile of the photosphere may contain quite as much matter as a cubic mile of hydrogen does here.

Let us suppose that each cubic mile in the photosphere draws in by a distance of 110 feet in the course of a year. It is quite plain that we have here a case where a multitude of weights are descending, and necessarily performing work of some kind or other in the course of their descent. Not only must the outer part of the photosphere shrink inwards, but of course the next layer beneath must sink inwards as well, though no doubt to a somewhat smaller extent. Each cubic mile of this second shell will be another weight, doing also its share of work during the progress of the shrinking. Let us conceive the process carried further still. Let us think that the whole volume of the Sun is divided into a series of shells, the radius of each shell being a mile greater than that one which it surrounds. Let us suppose that each shell is made up of a mosaic of cubical blocks, each one mile on the edge; then each of these blocks will in the course of a year shrink in towards the Sun's centre. The blocks on the outside will move more than the blocks in the inside, because if the Sun's diameter has to diminish by 220 feet—this being double the distance through which the radius contracts—there must be in some degree a proportionate shrinking in the several different shells. No doubt in the immediate vicinity of the centre where the spheres have become very small our conception of the subdivision into cubic miles might hardly apply. We can, however, think that the spheres are then divided into mosaics of a square inch, or for that matter of a square thousandth of an inch. Indeed, the argument will remain unaffected whatever be the way in which we regard the whole volume of the Sun as subdivided into parts, each quite small in comparison with the entire mass. Each of these parts draws in year by year towards the centre of the Sun, through a distance which is connected with its situation as regards the Sun's centre.

At first it might seem impossible to calculate the work that could be accomplished by the descent of so many masses. In reality, however, the task is quite within the compass of a mathematician. He can determine the total amount of work that would be done in the course of the year, and he finds that this can be expressed exactly by a certain number of foot-pounds of work. Availing ourselves now of the knowledge that 772 foot-pounds of work correspond to one unit of heat, we are enabled to determine the quantity of heat to which this condensation of the solar materials corresponds. It is thus found that with a shrinkage of 220 feet per annum, the total quantity of work transformed into heat would be sufficient to supply the Sun's annual requirements.

It should also be observed that this explanation of the source from which the Sun's heat is maintained is by no means at variance with that which I have given in the earlier part of the chapter. In reality there is no discrepancy between the two methods. The investigations we have just conducted show how, through the contraction of the Sun, a tremendous amount of energy becomes available for the production of work. But it does not exhibit the precise mechanism by which the transformation of that work into heat is accomplished. This is what the earlier part of our investigation has made clear. The energy generated by the descent of these masses towards the Sun's centre is mainly expended in one particular kind of work—namely, in adding to the velocities with which, on the average, the molecules of the solar gases are animated. As these velocities are increased the temperature of the gases tends to rise. This, then, is the source of that elevation in solar temperature which neutralises the loss of heat consequent upon radiation through space.

It appears from this discussion of the problem that the temperature of the Sun may remain absolutely constant during vast time periods. This is, however, a matter as to which we have no assurance, either from observation or from theory. If the heat generated by contraction in the way we have described were exactly the necessary amount to compensate for the losses incurred by daily radiation, then, of course, the solar temperature would remain invariable. If, however, the quantity of heat generated by the augmentation of the molecular velocity due to contraction should exceed the loss of heat due to radiation, then the temperature must on the whole rise. Of course it might also happen that though the production of heat by the transformation of mechanical effect was partly sufficient to neutralise the loss incurred in radiation, it might not be completely sufficient. If this be the case, then, of course, the solar temperature must be, on the whole, declining. Three conceivable states are, therefore, possible. All we can certainly say is, that whether the Sun's temperature be rising or falling or stationary, it is, at all events, so nearly stationary that no means of observation at our disposal have been sufficient to demonstrate the existence of any variations.

It is interesting to apply these principles to the study of the early history of our Sun. The contraction of the great luminary is undoubtedly going on at present, and it must have been in progress for many ages past. If, therefore, we look back through a vista of many thousands of years, we see the Sun ever growing in dimensions. It is now apparently contracting at the rate of 220 feet per annum. It must, therefore, have been 220 feet greater in diameter last year than this year; it must have been 220 feet bigger still the year before. I do not mean to say that the same rate of alteration has prevailed unchanged throughout the ages—that can certainly not have been the case; but, however far we strain our vision into the past, we see the Sun ever larger and larger the more remote the date. Nor, indeed, under the present laws of Nature as we see them in operation around us, can we discern any boundaries to this retrospect.

As to the original source of the Sun's heat, science can offer an explanation suggested by what is sometimes observed to happen in other remote parts of the universe. Let me give an illustration. In February, 1892, astronomers were astounded by the announcement that a new bright star had appeared in the constellation of Auriga. It was certain that this star could only have come into existence very shortly before it became a brilliant and conspicuous object. Photographic testimony left no doubt on this head. Plates on which this star showed itself as a brilliant object had been taken a few days subsequently to other plates depicting the same part of the sky from which this same star was absent. The distance at which the body was situated also made it manifest that the conflagration which this outbreak betokened could have had no insignificant proportions. Everything we have learned about the matter suggests that the new star in Auriga during the time of its greatest brilliance dispensed a lustre not inferior to that of our own Sun.

Nova Aurigæ appeared from our point of view in the guise of an insignificant star, by the circumstance that it was a million times farther away than the Sun. Spectroscopic examination threw some light on the character of this marvellous object. It became clear that the brightness of the new star in Auriga was the result of a collision which had taken place between two previously obscure bodies. Perhaps it would hardly be right to describe what happened as an actual collision. It is, however, perfectly clear from the evidence that two objects, whose relative velocities were some hundreds of miles a second, came into such close proximity that by their mutual action a large part of their energy of movement was transformed into heat, and a terrific outburst of incandescent gases and vapours proclaimed far and wide throughout the universe the fact that such an encounter had taken place.

Nor is Nova Aurigæ an isolated instance. Several cases are recorded in which collisions of a somewhat similar description appear to have taken place, the result being in each case announced by the exhibition of a new star. Such outbreaks are illustrations of the fact of which astronomers are becoming gradually cognisant, that there must be multitudes of dark bodies in the sidereal heavens—bodies which, under ordinary circumstances, are never visible. The word "star" seems inapplicable to either of the bodies forming Nova Aurigæ prior to their collision. I should rather describe it as a dark lump of matter—Sun-like it may be, as far as proportion and mass are concerned, though wholly devoid of brilliance.

Indeed, the theory of probabilities, so useful in the interpretation of Nature, would seem to assure us that these dark bodies exist in far greater profusion than even the bright stars. A star seems to be only a piece of matter which has been temporarily kindled by some accidental circumstance, so as to be enabled to diffuse light and heat. We can only see those portions of matter in the depths of space which happen to be bright in the particular ages in which the human epoch has been cast. The invisible stars, probably outnumbering the visible ones many thousands of times, cannot have their presence made known to us save in some indirect and casual manner.

Judging from the analogy of Nova Aurigæ and of the many other similar objects, it seems reasonable to suggest that the brightness of the Sun is due to a collision which occurred many millions of years ago between two dark bodies which, in the course of their wanderings through space, happened to approach sufficiently close together. We know that with such velocities as those possessed by the bodies travelling through space, an encounter of this kind would be quite sufficient to supply all the heat and all the light which the Sun now radiates. It has even been possible to go a step further than this.

In a lecture which Lord Kelvin delivered some time ago at the Royal Institution, he gave an illustration of a conceivable method by which a Sun resembling ours could have been manufactured by the collision of two bodies. He supposed the case of two globes, each with half as many miles in its diameter as the present diameter of the Sun; these globes were supposed to be cold solid bodies, each possessing the same mean density as the Earth. Suppose that these two globes were held apart at a distance equal to twice the Earth's distance from the Sun. If they were released, and if no other extraneous forces were in action, then the two globes would begin to draw together in virtue of their mutual attraction. At first, no doubt, the pace with which they moved would be extremely slow, but it would gradually improve as they came nearer to each other. It would indeed do so for a double reason. In the first place, as the distance between the globes lessened, the intensity of their mutual attraction would increase; in the second place, in accordance with the ordinary principles of mechanics, the speed which the bodies acquired in one second would be added to that which the force of gravitation imparted to them in the next, and so on. It is thus easy to demonstrate that six months after these two globes were started on their career they would fall together with an awful crash. A

PLATE X.

I. TOTAL SOLAR ECLIPSE, 29th JULY, 1878 (*Shumway*).

II. CORONA PROMINENCES AND CHROMOSPHERE IN THE ECLIPSE OF 1871.

marvellous transformation of work into heat would be the result. Have we not already explained how work is done when one body is attracted and moves towards the other? and as that work is equivalent to heat, it is simply a numerical calculation to determine the total quantity of heat that would be equivalent to the work done by the approach of these two globes to contact from their original distance.

The collision would doubtless last for hours, in the course of which the energy that we have referred to would be largely transformed into heat sufficient to render these two globes, or rather the materials of which they were composed, red hot and white hot. The originally cool bodies would finally be transformed into a violently agitated incandescent fluid mass, provided with a store of heat sufficient to sustain the current expenditure of the Sun at its present rate for a period of about eighteen or twenty million years. One immediate effect of this heat would be to expand the materials of the two globes, so as to form a single volume of gaseous or quasi-gaseous material, considerably greater than that of the Sun at present. The calculations we have just given, as well as our observation of the great events which actually take place in other parts of the universe, seem to make it clear that, in all probability, the Sun's origin as a luminous body may not have been widely different from that which has been now suggested.

CHAPTER XV.

THE IMPORTANCE OF CARBON IN THE SUN.

NOTWITHSTANDING all the labour that has been directed to the study of the Sun, notwithstanding all the facts that have been learned with respect to the spots and the faculæ, the prominences and the corona; notwithstanding that for more than a generation the light of the Sun has been so diligently scrutinised with the spectroscope that a multitude of interesting details have been brought to light, it must be admitted that we are still very ignorant concerning the actual physical nature of the great luminary. Theories of the Sun there are in abundance; some of them I have already touched upon elsewhere in this volume; I do not, however, propose to make any attempt at giving a full or complete account of such theories. I shall rather try to present a certain isolated fact in solar physics which appears indubitable, and which possesses special importance in celestial physics.

One of the most fundamental questions that can be asked with regard to the chemistry of the Sun relates to the nature of the materials in the photosphere. It is admitted that the copious radiation of both light and heat takes place from this particular layer of glowing clouds. It seems obvious that there must be some special material which chiefly, if not wholly, gives to the photosphere its marvellous radiating power. The light which we get comes not from within the photosphere, nor does it come from what lies without. The photosphere does not seem to be of any great thickness in comparison with the solar diameter. It is evident that if that particular shell of glowing clouds were absent the efficiency of the Sun as a radiator would be quite evanescent. It surely behoves us to ask what must be the nature of the materials with which these clouds are charged, or of which they are actually constituted, so as to give us the light and heat to which we owe so much.

Our own clouds floating in the atmosphere surrounding our globe would give a character to our globe as viewed from outside.

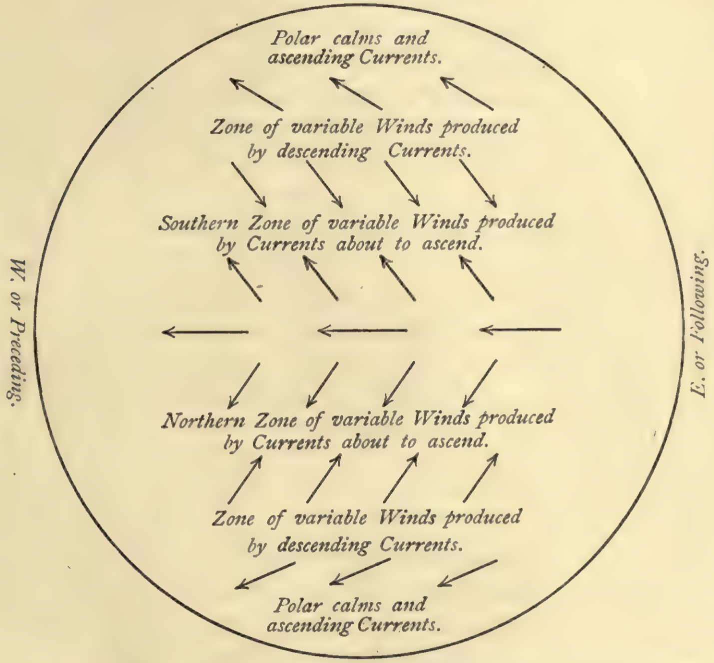


FIG. 68.—DR. G. JOHNSTONE STONEY'S THEORY OF THE CURRENTS IN THE SOLAR ATMOSPHERE.

We know, of course, that these terrestrial clouds arise from a single chemical compound—water. Can it be that in like manner some special substance is predominant in the solar clouds, and gives to them their particular character?

So far as I know, the solution of this problem was first attempted by Dr. G. Johnstone Stoney in the very remarkable paper on the "Physical Constitution of the Sun," read at the Royal Society on May 15th, 1867. The results at which he arrived have not, so far as I can learn, been invalidated by any subsequent researches. It seems to me that the solution he has offered must be the correct one. I shall here endeavour to set it forth, and to elucidate his argument as clearly as I can.

In the first place it may be observed that at the exalted temperature of the photosphere it is unlikely that the photospheric material, whatever it may be, should be any compound. It is known that chemical compounds tend to be resolved into their primary ingredients at the fierce heat to which they are exposed in the electric arc, while at the far greater fervour of the photosphere it would seem impossible for most ordinary chemical compounds to subsist. Dissociation must supervene. Nor is it difficult to see the physical reason why the component elements fly apart under the influence of excessive temperature, now that the molecular theory has taught us something with regard to the composition of matter. Each particle of matter consists of molecules in a state of rapid oscillation or motion. As the temperature rises, the velocity with which the molecules are animated increases also. The internal agitation of the molecules also increases, as is evidenced by their increased capacity for radiating heat. The consequence is that the atoms united to form a molecule of a compound body have their velocities so greatly increased as the temperature rises, that they become impatient of the restraints which their union imposes; so that when at last the temperature has reached a sufficiently high point, the atoms finally rupture the bond, and go their several ways. Thus it comes to pass that at a temperature like that which reigns in the photosphere, dissolution of chemical bonds seems generally to have taken place.

This consideration narrows the search for the body to whose peculiar properties the incandescence of the photosphere is due. We have not to consider the infinite variety of possible chemical compounds. We may restrict our investigations to the elementary bodies themselves. Of these elements there are, as we know, about seventy which are present in the Earth. Many or most, if not all, of them are also in the Sun. We have also good reason to think that there are two other elements at least in the Sun which are foreign to our terrestrial chemistry. These elements have received the provisional names of coronium and helium. But there is no reason to suppose that the glories of the photosphere are to be attributed to the presence of either coronium or helium. Indeed, all the evidence seems to show that these two bodies, if they exist at all, are of a character resembling hydrogen, only perhaps far lighter and more subtle than that element, which is itself the lightest body known on the Earth. None of the properties of coronium or helium resemble the properties we must attribute to the photospheric matter. Nor need we suppose the existence of some other peculiarly solar element in addition to those named. Such a course will not be necessary at all events until after we have carefully scrutinised the list of terrestrial elements, and seen whether we cannot there find some one or more substances which seem to possess the properties required. It must be remembered that many of the lines in the solar spectrum have been identified with those produced by terrestrial elements. It is thus probable that the Sun is chiefly composed of elements already well known to our chemists. It is further plain that the photospheric element, whatever it may be, must be very abundant in the Sun. It is, therefore, highly probable that this particular body belongs to our Earth as well.

We shall be guided in our search for the solar emissive element by considering the conditions in which the materials of the photosphere must exist; this will limit the direction in which we must look. In the first place it is to be noted that the excessive radiation from this particular solar region demonstrates that the emissive substance, whatever it may be, is not in a purely gaseous state. The continuity of the spectrum from the photosphere is unbroken. It is true that in the spectrum as we see it there are multitudes of dark lines. But these owe their origin to vapours in the solar atmosphere outside the photosphere, and indeed some of them arise, as we have already had occasion to mention, from our own atmosphere. We thus see that the spectrum of the photosphere would be a continuous band of light were it not for the interposition of absorbing media. We know, however, that a purely gaseous material, no matter how high be the temperature to which it is raised, will not produce a continuous spectrum. The appearance which its radiation presents when viewed through the spectroscope is that of a number of vivid lines. These are due to the agitations impressed upon the ether by the several oscillations that take place within the molecules. The spectrum of a solid or of a liquid rendered incandescent by heat is, generally speaking, of a different character. It forms what is called a continuous spectrum. In this case it would seem that undulations are produced and imparted to the ether, not merely in periods corresponding to those in which the several internal vibrations of the molecules are themselves performed in the comparatively free molecular condition characterising the gaseous state. The vibrations imparted to the ether by a glowing solid or a glowing liquid embrace periods of every degree, from below the red on one hand to beyond the violet on the other. This can only arise from the greatly increased complexity of the vibrations of a solid or a liquid as contrasted with the vibrations of a gas. In the grosser forms of matter the molecules are so hampered by their association with surrounding molecules, that the vibrations they perform are affected by multitudinous disturbances, setting into action every type of undulation within the specified limits.

Of this complex type are the vibrations of the photospheric material, and we must consequently suppose that the luminous matter is in the form of a solid or a liquid. Of course it is not to be supposed that the photosphere is a continuous mass of material either solid or liquid; such a notion is at once excluded by the incessant motions which the photosphere exhibits. Nor is such a supposition necessary. All that is required is that the radiation shall take place from particles which are sufficiently large to contain a great number of molecules. If the photosphere were a cloud consisting of myriads of dust particles, or myriads of droplets sufficiently heated, then the light that it would dispense must possess the actual qualities which we see it to have.

We have therefore reached a stage in the argument in which it has become plain that the photosphere must be composed of a shell of cloudy or vaporous material consisting of a myriad of particles, either solid or liquid, held in suspension. Now what is this material to be? Seeing that most of the metals proclaim their presence in the Sun by multitudes of lines in the solar spectrum, it is natural to inquire whether these photospheric clouds are due to the presence of any metal. Let us take for instance the element, iron; it seems quite easy to demonstrate that this cannot be the luminous material; for the vast abundance of the lines of iron in the solar spectrum shows us that iron abounds in the part of the Sun exterior to the photosphere. This iron exists in the form of gas or vapour, otherwise the dark lines could not be produced. It therefore follows that outside the photosphere the temperature of the Sun must be so great that it volatilises iron. But we are assured that the several successive layers or shells of solar material must become, generally speaking, hotter and hotter the nearer the centre is approached. This gradual increase of temperature from without inwards is a necessary part of almost any theory on the subject of the Sun's nature. It therefore follows that the temperature of the photosphere must be on the whole higher than that of the solar atmosphere, which lies exterior thereto. It is, however, in this outer atmosphere that the vapour of iron abounds. If therefore the temperature of these outer regions is sufficient to maintain iron vapour in abundance, it follows that the temperature of the photosphere must exceed that at which iron passes from the liquid to the gaseous condition. This being so, it seems obvious that the photosphere cannot itself consist of an iron mist, for the simple reason that the Sun at that depth is so hot that the mist would be transformed into vapour.

In like manner we can show that the other metals can hardly be the materials of the photospheric clouds. No doubt platinum is much more difficult to fuse than iron. It seems therefore probable that clouds of platinum-mist might exist under temperatures far too high to permit of iron vapour assuming a similar form. But there are other objections which render it certain that the photospheric clouds could not consist of platinum-mist. In the first place the rarity of platinum here seems to extend to the rest of the solar system. The low density of the Sun makes it very unlikely that a metal which is 21·5 times as heavy as water should form any considerable proportion of its mass. The high atomic weight of platinum, 194·9, makes it highly improbable that, even if such a material were present, it could raise itself to any considerable elevation in the solar surroundings. We may also add that the photographic researches of Rowland render it doubtful whether platinum exists in the visible parts of the Sun.

On these grounds we conclude that the photospheric clouds do not consist of metals, and in our search as to what they must consist of, we are led to consider the metalloids. It seems certain that the clouds must take their rise from some one or more of the following list of substances:—Boron, carbon, phosphorus, selenium, silicon, sulphur, oxygen, hydrogen, nitrogen, chlorine, iodine, bromine, fluorine.

The last seven of these may be at once discarded. They are all either gaseous bodies in their normal condition at terrestrial temperatures, or easily driven off into gas at temperatures greatly below that of the photosphere. Much the same may be said of sulphur and selenium and phosphorus. We are therefore reduced to the consideration of carbon, silicon, and boron. The last of these is a comparatively rare element, and in other ways it does not seem to fulfil the required conditions. We are thus led to the belief that among the elements known to us, carbon or silicon, or both, appear to be the most probable constituents of the photospheric clouds.

We therefore turn with much interest to the spectroscopic evidence bearing on the presence in the solar atmosphere of the two bodies named. Here, as in other departments of solar physics, the classical labours of Professor Rowland at the Johns Hopkins University, Baltimore, are our chief guide. He has demonstrated that, among the multitude of lines in the solar spectrum, there are about two hundred which are certainly due to carbon. Other lines indicate the presence of silicon. It is thus plain that the vapours of both these elements are present in the solar atmosphere. This demonstrates that the temperature of the absorbing atmosphere must in some places, at all events, be so high that it suffices to maintain carbon and silicon in the vaporous form. No doubt the arguments referred to in the case of iron might be here again brought forward.

It might be contended that the existence of the element in gaseous form disclosed a temperature so high that it would be impossible for the same element to exist in a cloudy state. The case of our atmosphere is here very much to the point. There are clouds of watery particles suspended therein, and there is also watery vapour. Doubtless there is a trace of vapour in the air above the clouds. We thus see that a vapour may exist in small quantities in the upper regions of an atmosphere, while at lower regions, even when the temperature is presumably much higher, the same element may exist in the form of cloud.

It thus seems obvious that it is the most infusible substances which are present in those solar clouds which give the continuous spectrum. Carbon and silicon are both only fusible at a temperature transcending that which suffices to fuse metals, and consequently it would seem certain that one or other, or both, of these bodies must be components of the photosphere.

Once the search has been narrowed down to a choice between two elements, it seems hardly possible to doubt which of the two it is that forms the material required. Here again we follow the extremely instructive line of reasoning introduced by Dr. Stoney in the paper already referred to.

Considering the enormous difference between the circumstances under which the elements are present in the solar atmosphere, and the circumstances under which the elements are present in our laboratory experiments, it seems impossible for us to comprehend the action that goes on in the Sun except by an appeal to the laws of molecular physics. Dynamical truths prevail under all circumstances, and when chemical or physical investigations can be conducted by the light of dynamics, we obtain a certainty which we feel is not impaired by the tremendous change in all the conditions of the problem.

The principle which is at present applicable is that which connects the velocities of the molecules of a vapour or gas at a given temperature with its atomic weight. Thus, to take the case of hydrogen, which we shall suppose to be at the temperature of freezing water, it can be shown that the average velocity of the molecules is 1,859 metres per second. Lest there be any uncertainty in the matter, I ought perhaps to explain that by average velocity we mean that particular quantity which is the square root of the mean of the squares of the actual velocities. In the case of oxygen the mean velocity of the molecule at the same temperature is 465 metres. This quantity is one-fourth of the former. But the atomic weight of oxygen is sixteen times that of hydrogen. Thus we see an illustration of the law which may be stated quite generally, that the velocity of the molecule at a given temperature varies inversely as the square root of the mass of the gaseous molecule.

Provided with this principle, we are now able to determine the mean velocities of other gases. It is true that the figures actually mentioned refer to gases at the temperature of freezing water. The absolute velocities of the molecules no doubt increase with their temperatures, but their relative values must always be consistent with the law just stated. At present we are concerned with the relative velocities only.

The atomic weight of carbon is twelve, and that of silicon is twenty-eight. The square root of the ratio of these quantities is 1·53. This is therefore probably the ratio of their molecular velocities. In other words, the carbon molecules will have on the average a velocity 1·53 times greater than the silicon molecules, both being at the same temperature. We thus see that the molecules of carbon are animated on the whole by a high molecular velocity relative to those of silicon. It will not be difficult to show that solar clouds must be specially likely to form when the material producing them is, on the one hand of a very infusible character, while on the other hand it possesses a high molecular velocity.

The elevation to which gaseous molecules ascend in the solar atmosphere must depend, other things being equal, on the velocities with which they are animated. The demonstration of this fact, which was first given by Dr. Stoney, involves an important principle in celestial physics generally.

Let us, for the sake of clearness, first suppose a rigid globe surrounded by an atmosphere constituted from a single element only, and consider the phenomena presented at the boundaries of this atmosphere. What, in fact, does constitute the boundary of an atmosphere? Is there, rather, any boundary at all? These are questions which can only be answered by the help of molecular physics; for think of the condition of affairs which must exist at the summit of an atmosphere. If there be a boundary, then it would appear that the molecules must occasionally dash across it into outer space. The attraction of the sphere will tend to bring them back; and if it should happen that the velocity with which they are animated does not exceed a certain critical value, each molecule will return again to the sphere. Thus the upper surface of the atmosphere, so far from being a definite stratum, will be constituted by a multitude of trajectories of particles which project themselves aloft and then drop down. It is obvious that the greater the velocity of the molecule, the higher it will be projected. Thus, under like conditions of temperature and gravitation, an atmosphere of a gas with a low atomic weight will ascend to a greater elevation than an atmosphere of gas with a high atomic weight. In the case where the atmosphere consists of two gases intermingled, the tendency at the limits of that atmosphere will be for the lighter molecules to leap outwards and upwards.

The low atomic weight of carbon (twelve) shows that this element will be constantly striving to elevate itself in the solar atmosphere. On the other hand, the higher it gets the lower is the temperature to which it is exposed, and, consequently, the greater is its tendency to assume a liquid or solid state. For we must bear in mind that carbon is an element so very refractory that the most intense heat we can produce barely suffices to render it vaporous. Note, then, how the two conditions of refractoriness and low atomic weight conspire to produce carbon clouds. Owing to the low atomic weight, the carbon vapours climb upwards. Thus they attain lower temperatures, which, though perhaps not low enough to reduce other vapours to the form of liquid drops, are low enough to reduce to the liquid or solid form a material which, like carbon, is so prone to return thereto.

All that our terrestrial experience has taught us with regard to carbon will serve to render it the more probable that to this element belongs the distinction of being the main source whence sunlight is dispensed. It is the presence of particles of carbon in the flame of a candle that gives to it its illuminating power. We employ carbon for the filaments of our incandescent lamps because this element is so refractory that it will remain in the solid state at a temperature far exceeding that at which even platinum itself would have been fused. It is this same property of carbon which renders poles of this element so indispensable for the production of the arc light. Carbon is the most efficient of radiators; it is therefore specially adapted for the scattering abroad of that light which it receives from the passage of the electric current. But even its admirable capacity as a radiator would be of but little help were it not for its refractory nature. It will stubbornly remain solid even though exalted to an enormously high temperature. This imparts to it the means of utilising its great radiating power. And it has the third requisite property in its low atomic weight, which enables its vapours to keep constantly diffusing upwards into those higher portions of the Sun's atmosphere, where the temperature is such that the vapour becomes cloud according as it arrives. On these grounds Dr. Stoney has concluded that in all probability it is to the element carbon that we are directly indebted for the light from the photosphere. His arguments appear to me to be unanswerable.

Dr. Stoney's theory has also led him to the view of the winds and currents in the solar atmosphere which is represented in Fig. 68; but it is not possible for us to enter further into the discussion of these elaborate inquiries.

CHAPTER XVI.

THE SEASONS, PAST AND PRESENT.

IN Fig. 69 we show, in perspective, the several positions of the Earth in its annual voyage around the Sun. On the 21st of June the northern hemisphere is turned towards the Sun. It consequently receives an undue share of Sunbeams, and summer reigns. Six months later, on the 21st of December, the north pole is turned away from the Sun. Winter accordingly prevails in that hemisphere. It is then that the south pole is turned towards the Sun, to enjoy those benefits of light and heat which give it its turn of summer. The actual shape of the orbit is perhaps better seen in subsequent figures, in which the path is viewed squarely, so to speak. It need hardly be remarked that the size of the Earth has been monstrously exaggerated in comparison to the size of the orbit.

If the causes of the variations in climate were exactly as we have described them, the succession of summer and winter might remain eternal. It is, however, important to notice that there are certain causes of change whereby the climates of our globe have not always been just the same as they are at present. We admit, of course, that our climate appears to be constant when we only bring into view such centuries or thousands of years as those with which human history has to deal. There is not the slightest reason to believe that the present climate of Great Britain differs in any recognisable way from the older climate in the time of Queen Elizabeth. Indeed, we must look back to a period much earlier still before we meet with evidence of any marked alteration in the character of our summers and winters. William the Conqueror found those seasons in England to be of much the same description as we find them now; nor is there any ground for the belief that even so far back as the time of the Romans, the seasons differed, to any noticeable extent, from the seasons in the England of our present monarch.

But in the consideration of such a question as that which is now before us, we must stretch our retrospect to an epoch far earlier than any to which our known historical records extend. It will presently appear that in the very early days thus indicated there have been immense variations of climate. It is true that from the nature of the case no direct testimony can be available as to what the character of the climate may have been at dates far anterior to any human monuments. But we must not, therefore, conclude that testimony of the kind required is altogether wanting. Indeed, we have a volume of evidence which demonstrates in the most conclusive manner that our Earth has undergone tremendous changes of climate since the time when it first became the abode of life.

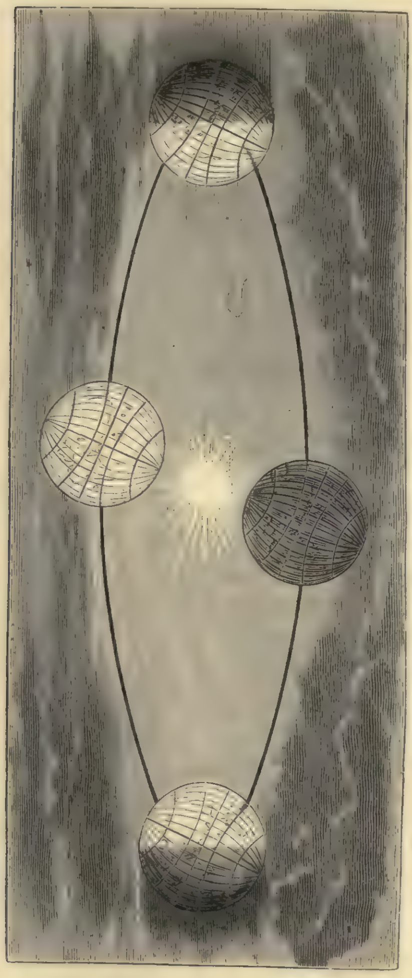


FIG. 69.—TO ILLUSTRATE THE DIFFERENT SEASONS.

Although it is certainly true that the latest of these great climatic changes is believed to have been prior to the earliest dawn of human history, yet we must not, therefore, infer that it was altogether prior to the human period itself.

There is, in fact, sufficient reason for thinking that certain vast alterations of the kind have occurred since man took his place upon the Earth. Implements of stone, unquestionably of human manufacture, have been obtained under conditions which seem to render it highly probable that man and the more recent of the great climatic changes were contemporaneous. I should, however, warn the reader that the question as to how far the vicissitudes we have to describe were concurrent with the human period is a matter about which there is still considerable difference of opinion. Fortunately for our present purpose, this aspect of the question is immaterial, and need not be again referred to.

The testimony as to pre-historic climate, on which we have mainly to rely, is derived from the study of geological phenomena. I may just offer one or two samples of the evidence for our belief as to the extraordinary fluctuations of climate which our Earth has undergone; nor will it be necessary to go outside the limits of the United Kingdom to find abundant illustration of the facts desired. Consider, for instance, the conditions under which beds of coal must have been originally formed; it is perfectly obvious that our fuel is the remains of a luxuriant vegetation which at certain periods of the Earth's history flourished where the British Isles are now situated, as well as in many other parts of the globe. No doubt coal is, generally speaking, so much altered in appearance from the primeval vegetation that it is not often easy to determine by its structure from what description of plants it was derived; enough, however, is known to enable us to assert that the primeval forests must have largely consisted of ferns or of plants of an allied character which flourished in an abundant luxuriance that no vegetation in these latitudes can nowadays emulate.

I do not, indeed, assert that the conditions necessary for the production of coal were of the same description as those which we find at present in tropical regions where vegetation is so rampant. From the study of the flora of New Zealand, and of other similar countries, it might almost seem that the requirements for the growth of a coal forest were rather provided by a mild form of climate, in which extremes both of warmth and of cold were absent, than by a fierce heat like that which our present Equatorial forests receive. But it is not necessary for us to discuss at any length what the character of the climate of the carboniferous period may have been; it suffices for our present object to note that in some most essential features it differed radically from the climate that we now find in Great Britain.

The lesson derived from the coal is confirmed by what we learn from many fossil organisms of very varied type. There is, for instance, a remarkable tribe of fossil shells known as the ammonites, which are allied to the nautilus of our present seas. The creatures that once inhabited these shells resembled the modern cuttlefishes, which, under certain conditions, attain vast dimensions. In certain beds of rock the ammonites are extremely abundant, both in number and also in the variety of species which they represent; sometimes they are of colossal dimensions. There are some of these fossil nautili which measure as much as four feet across. It is clear that these organisms must once have lived in the seas that flowed over the position now occupied by the rocks in which their remains are found so abundantly. But from all we know of the habits of such creatures, it seems impossible to believe that they could have thriven in waters so chilly as those of the oceans in these latitudes. It seems hard to resist the conclusion that, from some cause or other, the waters of the seas in which the ammonites abounded must have been more genial than the North Sea of the present day.

The illustrations I have given happen to indicate higher temperatures in the past than in the present; but it would be quite erroneous to suppose that the geological evidence respecting ancient climates is all of this character. In fact, nothing could be more certain than that there was a time when the climate of Great Britain, instead of being milder than it now is, was far more rigorous; indeed, it seems difficult to doubt that the present conditions of Greenland were in some measure paralleled by climates formerly experienced in regions which are now in the temperate zone. I propose to enter with some detail into the question as to the cause of these cold periods through which our Earth, or perhaps I ought to say our hemisphere, has certainly passed. It should be noted that, from the geological point of view, at all events, the phenomena we are to consider are of quite a recent character. No one can say how many thousands or millions of years have elapsed since the coal beds were deposited, or since the ammonites vanished; but of this we may feel certain, that the time was much more remote than the comparatively modern epoch when the Earth was last in a glacial condition. In fact, from the standpoint of geology, the phenomena of glaciation must be regarded as but of yesterday. The great beds of rock that form the Earth's crust had been deposited, the great chains of mountains had been upheaved, the seas and oceans had occupied their present basins, when that vast climatic change took place which geologists call the great Ice Age. So recent was this period that the corrosion of time has not yet sufficed to obliterate the widespread records of its presence left by the ice. Hence we are enabled to collect testimony over large parts of the world as to the frightful severity of the conditions accompanying the great Ice Age. Let me just mention a few of the different classes of monuments from which our information has been obtained.

In many parts of the country mighty blocks of stone may be frequently observed perched on a hill-side or occupying some other conspicuous position. Such stones often differ in their composition from the rocks which lie beneath them, and in many cases it has been possible by an examination of the stone itself, and by a careful study of the surrounding country, to ascertain the source from which the object must have come. A boulder, as it is generally called, will often weigh as much as 100 tons, or in some cases considerably more; and only some gigantic power could have been sufficient to rend this stone from the home where it was originally placed, and convey it, perhaps many miles, to the spot where it now lies.

A visit to a Swiss glacier suggests at once the method by which the transport has been effected. As the river of ice wends its way slowly down the valley, it frequently happens that blocks of rock from the overhanging cliffs tumble down on the icy surface, and these stones accumulate on the glacier in sufficient numbers to form the moraines so well known to every Alpine explorer. In this way a boulder may be carried for many miles from where it originally started.

Sometimes the glacier, moving slowly, but with irresistible power, may wrench out portions of rock from the bed over which it travels and bear them along with it, to be deposited in the terminal moraine at the foot of the glacier. Now suppose that some great climatic change were to occur by which a glacier which had existed for thousands of years should be made to vanish. The materials of its moraines would be found strewn hither and thither through the valley, and it might be possible by a careful study of the disposition of these rocks to form some conception of the volume of the former glacier, and of the extent of country which it covered. Here we have the true explanation of the origin of those great boulders which lie so numerously over many parts of our temperate regions. They were borne to their present destinations by ice; that ice has doubtless long since vanished, but the boulders are monuments which testify unmistakably to its former existence.

In the Ice Age, however, a large part of our present temperate regions was covered far more completely with ice than Switzerland is at present. It is rather to the present state of the interior of Greenland that we should look for an illustration of what Scotland and Ireland and the northern parts of England must have been like during the great Ice Age. The exact extent of the ice-sheet may be a matter of some uncertainty; but a large part of the country seems indisputably to have been overlaid with it. The occurrence of this desolating invasion is manifested by the operations which it effected. As the sheet travelled slowly but continuously over the surface, it ground and crushed the rocks which lay beneath. In consequence of this action, quantities of pulverised material were manufactured, and a cushion of clay and stones became interposed between the ice-sheet and the rocky pavement. When in process of time an amelioration of the climate took place, the great ice-sheet vanished, but a sheet of what we call boulder clay remained to testify to its former existence.

Over a large part of Great Britain we find layers of such clay, charged with stones either sharply angular, or more or less worn. These stones frequently exhibit, by scratches and other marks, some traces of the hard treatment they received in former days, when the clay in which they lie was kneaded between the ice-sheet and the rock beneath. Nor are other indications of the former presence of the ice-sheet wanting. It can easily be imagined that as a sheet of ice, perhaps a thousand feet thick, moved over the face of the country, it must have acted as a vast sheet of sand-paper, rubbing off the irregularities and prominences of the hills, and imparting to them a rounded and flowing outline. Such evidences of ice-action may be actually observed in many parts of our mountainous regions. But there are still other indications of ancient ice of a very unmistakable kind. They are to be met with in such localities as the Vale of Llanberis, where the sides of the hills are frequently grooved and scored by the passage of now vanished glaciers. Doubtless in such icy rivers projecting corners of large blocks of stone would act like graving instruments, and inscribe their marks on the channel down which the ice was flowing.

I have only alluded to the evidence afforded by Great Britain, but it is quite plain that at the time of the great Ice Age a large part of the temperate regions of the northern hemisphere was submerged beneath an ocean of ice, which often overflowed the summits of lofty mountains. It would almost seem as if the ice, which is now restricted within the Arctic regions, was in the days of the Ice Age permitted to advance southwards in all directions. It spread down over a part of Siberia, it desolated the northern parts of Europe, and encroached upon North America nearly as far as Washington. For thousands of years probably, some of the fairest parts of our temperate regions were desolated by this icy invader, but at last a change in climatic conditions supervened. The ice gradually retreated to its confines in the Arctic regions, leaving behind it on all sides those traces of its handiwork which we have here endeavoured to indicate.

It is naturally a problem of extreme interest to discover the cause of so tremendous a fluctuation in the climate of our globe. We are certainly justified in assuming that to produce an effect of such magnitude an agent of corresponding proportions is demanded. Various efforts have been made to assign an adequate cause for the production of the observed phenomena. I will not, however, enter any further into debatable questions than may be necessary to make the astronomical explanation of the Ice Age sufficiently clear.

In the first place it must be noticed that as the Sun supplies to the Earth all the heat that it obtains from the outside, it is impossible to exclude the orb of day from any consideration of the causes which have produced climatic fluctuations. Indeed, the very fact of the recurrence of Ice Ages might make it almost seem as if the Sun himself dispensed his beams in some inconstant manner. No doubt this consideration suggests an heroic way of solving the problem. There are, however, fundamental objections to the supposition that an Ice Age arises from direct changes in solar intensity. In the first place, it is absolutely certain that the capacity of the Sun for radiating heat is subject to no variation that could furnish the cause of an Ice Age. We must also remember that ancient climates have shown periods of unusual warmth as well as of unusual coldness; and that if we supposed a chilling down of the Sun to explain one of these periods, we should require an undue exaltation of its temperature to explain the other. We may say at once that there has been no such flickering of the Sun's heat as these suppositions would require.

I may also add that in all probability both hemispheres of the Earth were never in a glacial condition together. In fact, we may state the matter even more strongly; for there is excellent ground for the belief that a glacial condition of one hemisphere must have been accompanied by an unusually genial period in the opposite hemisphere. This would, of course, be wholly incompatible with the notion that the Ice Age was due to the lowering of the Sun's radiation, because then both hemispheres should have experienced their Ice Ages simultaneously. This latter consideration will also dispose of the notion that an augmented distance from the Earth to the Sun would serve to explain the advent of an Ice Age. No doubt the distance between the two bodies is a very material element in the question, but it would not be by any means correct to assert that during the thousands of years that an Ice Age lasts, the Sun is, on the whole, further away from the Earth than it is during the thousands of years when the hemisphere under consideration enjoys a genial temperature. No doubt an increase of the mean distance of the Sun would greatly reduce the temperature of the Earth. If, for instance, our globe revolved at a mean distance from the Sun ten millions of miles greater than it is at present, there would be so great a fall in temperature that a large part of the present temperate regions of the globe would certainly be rendered uninhabitable by extreme cold. But this would affect both hemispheres alike, and would not, in fact, help us to the solution of our present problem. It is, however, useless to discuss the matter, for we must not introduce a hypothetical agent of climatic change for which we have no warrant in the actual facts of astronomy. We know perfectly well from the laws of nature that the distance of the Sun could not vary in the manner just assumed; so nothing further need be said on that question.

It is only of late years that we have become fully aware of the true explanation of an Ice Age; and the essential principle of the matter is so simple that I must endeavour to set it forth in these pages. I have already mentioned that the Earth revolves round the Sun in an elliptic orbit, the dimensions of that ellipse and the various circumstances of its position being all perfectly known to us. If the Earth and the Sun were the only bodies in our system, then the orbit which our globe follows would never vary from year to year; it must, in fact, be the same at the end of each thousand of years as it was at the beginning; summer and winter, seedtime and harvest, would succeed each other with no greater diversity than they show at present. But now introduce the consideration of the other planets which also revolve round the Sun. In virtue of the law of gravitation, the Sun, of course, attracts each of the planets, and the planets attract the Sun and attract each other. In consequence of these attractions there are so-called perturbations in the movements of each planet arising from the forces with which the other planets solicit it.

The investigation of the effects of these perturbations opens up a most recondite branch of mathematical research. Assuredly we have no intention of now entering into this aspect of the subject. It fortunately happens, however, that the results which we require admit of being stated in a very simple and definite manner, even though we are not able to indicate the nature of the demonstration by which they are established.

To simplify the matter, let us suppose the case of two planets, the Earth, for instance, and an outer planet such as Jupiter. Of course, as these planets, however intrinsically vast, are, nevertheless, small in comparison with the Sun, the utmost perturbing effect which they could mutually produce must necessarily remain small in comparison with the main solar attraction by which their movements are guided. We need not, therefore, expect that the orbits will be disfigured to any large extent by perturbation. The Earth's orbit is now nearly a circle, and nearly a circle it will always remain. I have, however, in the adjoining diagram (Fig. 70) slightly exaggerated the degree of ellipticity which the orbit can assume. Indeed, if the figure were drawn accurately to scale, it would be hard to illustrate the

FIG. 70.—TO ILLUSTRATE THE PERTURBATIONS OF THE EARTH'S ORBIT.

argument effectively. I shall, however, take care, before the close of the chapter, to remove any erroneous impression that might otherwise be produced.

In the first place, we have to note that by a very beautiful law, discovered by Lagrange, the major axis of the Earth's orbit, as a mathematician would style it, never undergoes any change of appreciable amount. Whatever other alterations, therefore, the orbit has to undergo, the distance mentioned remains unchanged. The effects of the attraction of Jupiter will be shown by a corresponding alteration in the shape of the Earth's orbit. The present nearly circular form will become more oval. It is true that this modification is effected with extreme slowness; in fact, we may say that hundreds of thousands of years must elapse before the Earth's orbit will exchange its present form for the most highly eccentric ellipse that it is capable of becoming. Even then its departure from the circular shape will not be very conspicuous. We thus see that the result of the attraction of one planet on another is to transform a circular orbit slowly into an ellipse, and then back again into a circle by an oscillation that never ceases. The limits of change are, however, always very small, and the time required for each oscillation is enormously long. In the case of the Earth, other planets besides Jupiter contribute to the alteration in the shape of its orbit, the net result being that while the Earth's orbit does not vary as to the length of its major axis, it submits to a certain small periodic variation in its eccentricity. To these changes in the shape of the path which the Earth pursues around the Sun, we must attribute the Ice Age.

At the first glance it might appear that, considering how small is the actual variation of the orbit which the attraction of the planets is competent to produce, it cannot conceivably be adequate to account for phenomena so stupendous as those associated with an Ice Age. It is, no doubt, quite true that from one point of view the cause in question does seem an insignificant one; but we must remember that the product of two numbers may be very large, though one of the numbers be excessively small, provided that the other number be sufficiently great.

We have mentioned one factor, so to speak, in the production of an Ice Age, and it is, we admit, a small one. Now we have to mention the other factor, and it is so potent that the two together are quite sufficient, when united, to explain even so vast a phenomenon as the advent of an Ice Age. For consider all we owe to the heat which we receive from the Sun. We do not perhaps always realise the depth to which the Earth's temperature would fall were his beams to be extinguished. It is, of course, the heat of the Sun which preserves the surface of the Earth from sinking to the temperature of space. What that temperature may actually be is a question about which there is some little uncertainty; it is, however, generally admitted that it must be at least 300 degrees Fahrenheit below zero, if not much lower still. We shall, at all events, be quite on the safe side in assuming that the solar radiation maintains the temperature of the Earth's surface at a point fully 300 degrees above that to which it would sink if the Sun were withdrawn. Provided with this reflection, it is easy to see that a change in the amount of the Sun's heat, which bore only a very small relation to its entire radiation, might still be large enough to produce a climatic effect of tremendous importance.

We may illustrate the matter in this way. Suppose that the heat from the Sun were to be diminished by one-tenth, it might at first be supposed that this need not necessarily involve any very significant modification of climate. We recall, of course, the great fluctuations of temperature which experience shows occur from one day to another. We may have heard how the temperature of Chicago has been known to run through a range of 80 degrees in twenty-four hours. With changes like this habitually taking place all over the Earth, what, it may be said, could the loss of a tenth of the Sun's heat signify? But then comes in the reflection that it is the function of the Sun to maintain our Earth 300 degrees higher than it would otherwise be. Severe as a temperature of 20 degrees below zero may seem when experienced in Chicago in January, it must be remembered that if it were not for the Sun the temperature would be further below even this point than boiling water is above it. The change of one-tenth in the Sun's radiation would involve a corresponding alteration in that supply of heat which at present maintains the temperature of the Earth's surface 300 degrees higher than it would otherwise be. We are not, it may be, justified in assuming that the temperature maintained is exactly proportional to the heat received, but, for the purpose of the present illustration, it may be assumed with sufficient accuracy that a decline of one-tenth in the solar heat would involve a proportional lowering of the mean annual temperature of the globe. Nor is it doubtful that such a degree of variation as we have supposed would be of tremendous climatic importance, inasmuch as it would amount to a decline of about 30 degrees in the mean annual temperature. A fall so great would transform our temperate regions into an Arctic desolation.

This illustration will, at all events, prepare us for the necessity of scrutinising with extreme care any discoverable causes of variation in the heat-supply transmitted to the Earth from the Sun. We may admit at once that in the Sun itself there has been no fluctuation of radiating power perceptible within such periods of time as those within which Ice Ages have been comprised. We may also admit that as the average distance from the Earth to the Sun is, fortunately for humanity, an invariable quantity, the total annual supply of Sunbeams may be regarded as a practical constant. In fact, it seems certain that at the time of the greatest Ice Age, the Earth received in the course of the year no less heat than it does at the present time. Indeed, if there be any difference between the two cases it would not be hard to demonstrate from astronomical considerations that the annual reception of heat during the periods of glaciation must have been slightly in excess of the amount of heat which we receive in these comparatively genial times. We must then seek the cause of glaciation, not in an insufficiency in the annual supply of heat, but in the irregularity of its distribution.

At this point we are obliged to introduce certain mathematical principles which it will be unnecessary for us to attempt to prove here. Fortunately, however, the results to which we are led are capable of enunciation without the use of mathematical symbols. I should mention that the theory we are about to explain is due in a great measure to the labours of one distinguished physicist, the late Mr. James Croll. It may, however, be proper to mention that the doctrine which justly bears Mr. Croll's name was in some respects inadequate. In the account here given I shall endeavour to set it forth as fully as its importance demands.

In order to prevent any misapprehension, it will be necessary to define clearly the meaning to be attached, in this discussion, to the words Summer and Winter. I shall count the summer to be the season extending from the vernal equinox to the autumnal equinox, the remainder of the year being, of course, the winter. We thus dispense, for the present, with the intermediate seasons of spring and autumn, for their introduction would only tend to confusion. If the Earth's orbit were strictly a circle, then it must necessarily follow that the summer and winter would be equal in duration, each being, of course, one-half the year. As, however, the orbit of the Earth is not circular, the necessity for the equality of the two seasons would, generally speaking, disappear. It could no doubt happen, even with a highly elliptic orbit, that the summer and winter might be of the same length, but this would be an exceptional condition of things. Technically speaking, it would arise when the line of equinoxes coincided with the major axis of the Earth's orbit.

The more usual aspect of the matter is shown in the adjoining figure of the elliptic path of the Earth. We may suppose the line P Q drawn across the figure to represent the position of the line of equinoxes. Then, while the Earth is moving from P to Q around the part of the path marked A, it is summer, we will suppose, in the northern hemisphere, and winter in the southern. During the remaining part of the journey from Q, round by B, and back again to P, winter reigns in the northern hemisphere and summer in the southern. The time taken by the Earth in passing from P to Q is, however, less than the time required to return from Q to P. There is, indeed, a double reason for this difference. In the first place it is sufficiently obvious, from the figure, that the length of the arc P A Q is less than half the entire circuit of the ellipse, while Q B P is more than half. So that even if the Earth's speed were uniform, there would be a difference in the length of the seasons. But the speed is not uniform. It follows, from Kepler's Laws, that the velocity with which the Earth moves while traversing the A part of the orbit, is, on the whole, greater than that with which it moves through the remainder. Not only, therefore, is the journey shorter in length, but it is performed at a higher rate of speed. Thus we have a double reason why the duration of the season while the Earth moves from P to Q is less than half the year. It will, however, be noticed that if the two equinoxes, instead of lying at the points P and Q, should happen to coincide with the points A and B, then the symmetry of the figure demonstrates that the time taken to describe one season would be precisely equal to that required for describing the other. This is the case already referred to, in which, notwithstanding the fact that the orbit is not a circle, there is still equality in the duration of summer and winter.

It is, however, necessary for our argument to consider the most extreme case that can possibly arise. This is represented in Fig. 71, in which it will be observed that the line of equinoxes P Q has been so adjusted as to cross the axis of the ellipse at right angles. The inequality in the duration of the seasons is now at its widest, for the Earth is moving with its maximum velocity while at A, and therefore much less time will be required to traverse the small arc P A Q than would be necessary to go round the long arc Q B P, during part of which, in the neighbourhood of B, the velocity of the Earth is indeed the slowest in the whole orbit.

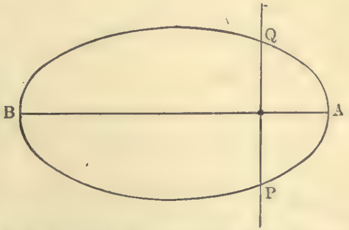


FIG. 71.—TO ILLUSTRATE THE UTMOST DIFFERENCE IN THE SEASONS.

The ellipse that I have here drawn is, no doubt, of a far higher degree of eccentricity than the Earth's orbit can ever actually possess. It will therefore be right to give the exact numerical facts which are material to our present purpose. We have already stated that the alterations in the form of the Earth's orbit are due to the attraction which our globe experiences from the other planets. We are enabled to submit this problem to mathematical calculation; it involves, no doubt, some very difficult work, but it admits of receiving an accurate solution. The result tells us the highest degree of eccentricity to which the orbit has ever attained under the solicitation of the different planets, among which Jupiter and Venus produce specially notable effects. This result is most conveniently expressed in terms of the number of days measuring the

FIG. 72.—CHANGES IN THE SEASONS.

difference between the lengths of the seasons, when all the circumstances conspire to make it as large as possible. We cannot be quite sure of the number to a day or two, but we shall be certainly within the mark if we assert that there have been times in the Earth's history when the difference between summer and winter reached as much as thirty-three days. The calculations demonstrate that one season has been sometimes as much as 199 days, while the other was only 166, the two together making up the 365 days of the entire year. It has, therefore, sometimes happened that the northern hemisphere has had a brief summer of 166, and a long winter of 199 days.

We know also that the changes have been so great that this condition of things has been occasionally reversed. Thus we find that the northern hemisphere has, at certain periods, enjoyed a long summer of 199, and a brief winter of only 166 days. It must also be noted that the time of summer in one hemisphere is, of course, the time of winter in the other; so that in those years when the northern hemisphere has a long summer and a short winter, the southern must have had a long winter and a short summer. We know, too, that transformations like those just indicated have occurred again and again. There has also been every gradation between seasons of absolute equality and those which attained that difference of thirty-three days which we have taken as a maximum. There are, indeed, certain facts which would seem to justify us in assigning to that maximum a duration some two or three days longer than that which I have actually adopted; however, for the sake of being on the safe side, I have taken the lowest value of the maximum of which the actual facts will admit.

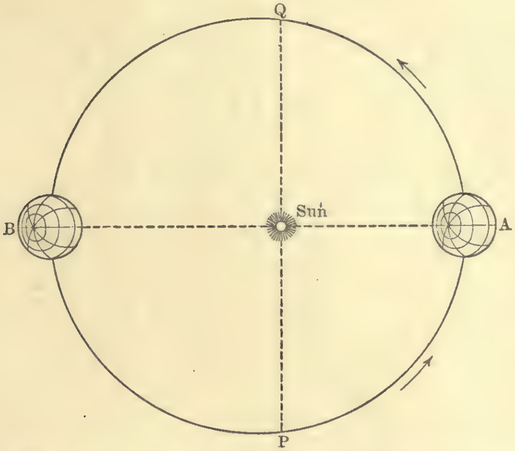


FIG. 73.—ACTUAL ORBIT OF THE EARTH WHEN THE NORTHERN HEMISPHERE IS GLACIATED.

We have now to show that these variations in the lengths of the seasons constitute one factor in the true explanation of an Ice Age. So long as the seasons remain nearly equal no such phenomenon as glaciation is possible. When the difference attains an amount no greater than, let us say, the seven days which is its present value, there is still no agency sufficient to produce glaciation to any appreciable extent. We may, however, remark that the existing difference of seven days tends to show that the southern hemisphere makes, in these present ages, some slightly closer approach to glacial conditions than the northern hemisphere. Doubtless seven days, or perhaps even twice seven, form a difference insufficient to produce any disturbance of climate comparable with that for the explanation of which we are now searching. But when, as from time to time happens, the difference in the length of the seasons approximates to its maximum value, the profoundest climatic modification results.

We have now to introduce a principle which lies at the heart of the matter, and is perhaps the most important law that can be enunciated with respect to the astronomical phenomena bearing upon climate. The theorem to which I refer is due to Wiener. I need not here state it under its most general form; it will be sufficient for our present purpose to present the theorem merely so far as it relates to the problem of glaciation. Let us think of one hemisphere of the Earth, and consider the quantity of heat which that hemisphere receives from the Sun in the course of a twelvemonth. We have first to note that each hemisphere receives an equal share—that is to say, that half the total supply of heat communicated to the Earth in a twelvemonth falls on the northern, and the other half on the southern hemisphere. This is true under all circumstances, and whatever be the difference in the length of the seasons. The immediate matter for consideration, however, is not the total quantity of heat which the hemisphere gets in a year, but the way in which the receipt of that heat is divided between the two seasons of which the year is composed.

To make the matter clear, let us suppose the terrestrial orbit exactly a circle. If the axis of the Earth were perpendicular to its orbit, or, to put it in the language of astronomy, if there were no obliquity of the Ecliptic, then it is perfectly plain that there would be no distinction of summer and winter, and the receipt of heat would be uniform on each hemisphere, and the same all the year round. As, however, the axis of the Earth is not perpendicular to its orbit, we have, of course, the distinction between summer and winter; and the amount of heat received during summer on one hemisphere is less than that received during winter on the same hemisphere. The amount of this difference depends upon the angle by which the axis of the Earth departs from the perpendicular to its orbit.

At the present inclination of the Equator to the Ecliptic, the ratio between the summer receipt of heat and the winter receipt of heat on one hemisphere may be thus stated. Let the total quantity of heat meted out to that hemisphere in a twelvemonth be represented by one hundred; then of this, sixty-three parts are received during summer, and thirty-seven during winter. If the obliquity of the Ecliptic were changed, these numbers would have to undergo alteration. It is, however, a noteworthy circumstance that the inclination of the Ecliptic to the Equator is one of those elements in the solar system which experience comparatively little variation. I do not mean to assert that this angle is absolutely constant. Indeed, it would be hard to name any astronomical element which could be so described. The obliquity certainly waxes and wanes within narrow limits, and when vast periods of time, commensurate perhaps with remote geological periods, are considered, it may be admitted that the value of this element has possibly undergone considerable changes. But within a period of the Earth's history so recent as that of the great Ice Age, it is practically certain that the angle between the Ecliptic and the Equator has experienced no noteworthy changes. We are therefore entitled to infer that the numbers sixty-three and thirty-seven sufficiently represent the receipt of heat in the two hemispheres respectively during that span of Earth history which concerns us at present.

We may now mention another consequence involved in Wiener's law. I have been supposing the orbit of the Earth to be circular, but even if it be an ellipse, the same numbers will still express the relative values of summer and winter heat, provided always that the obliquity of the Ecliptic remains unaltered. Still more significant is the further fact that these numbers will continue to express this great natural law wherever the line of equinoxes may happen to cross the orbit. In fact, let an ellipse be drawn with any degree of eccentricity, such as that in Fig. 74. Let the line of equinoxes cross it anywhere as from P to Q, then, provided only that the obliquity of the Ecliptic retains the value it has at present—namely, 23° 27′—the law will hold good. If it be the northern hemisphere which has its summer during the passage of the Earth through P A Q, then during that season the northern hemisphere will receive 63 per cent. of its total heat supply for the year, leaving only 37 per cent. for the winter in the same hemisphere, while the globe moves through the arc Q B P. The bearing of this law on our present subject will become apparent when we reflect that this distribution of heat is in no way connected with the lengths of the seasons. Whether the summer be long or whether it be short, precisely the same total amount of Sunbeams will be delivered in that season upon the hemisphere under consideration. All this is declared by Wiener's theorem, and I think it is hardly possible to overestimate its importance in elucidating facts that must otherwise be quite obscure with regard to climatic changes on the Earth. This doctrine has many applications in physics, and will have more when it becomes generally known; at present we are merely concerned to note the fact that it completes the long-sought explanation of the cause of an Ice Age.

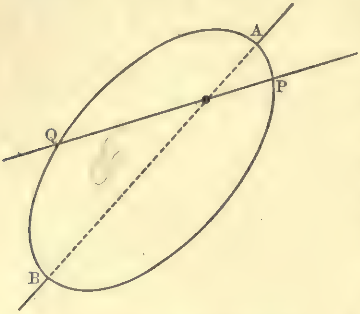


FIG. 74.—ELLIPSE TO ILLUSTRATE WIENER'S LAW.

We have already shown that under certain combinations of circumstances the summer may be as much as 199 days and the winter only 166. This applies alike to both hemispheres. But for facility of exposition let us fix our attention on the northern hemisphere only, merely observing that what we have to say with regard to it must apply precisely to the southern hemisphere also. During this long summer of 199 days, 63 per cent. of the total heat of the year is poured in, while the remaining 37 per cent. is expended in alleviating the severity of winter. In this case it will be noted that the larger of the two amounts of heat is apportioned to the longer of the two seasons, and the smaller of the two amounts of heat to the shorter season. This seems an appropriate disposition, and the resulting climate is as mild and beneficent as is compatible with the present obliquity of the Ecliptic. The summer is not extravagantly hot, nor the winter exceptionally cold. It is to be noted, in fact, that the character of the climate, which would be enjoyed under the conditions that we have supposed, would be much milder and more genial than that which we have at present. In these centuries we have, as already pointed out, an inequality in the length of the seasons which only amounts to seven days; the northern hemisphere has a summer of 186, and a winter of 179 days. Thus 63 per cent. of the annual heat is at present spread over a season not greatly differing in length from that which receives 37 per cent. of heat. The contrast between summer and winter, as now experienced in our northern hemisphere, is accordingly much more accentuated than it must have been in those periods of the Earth's history when the summer lasted for 199 days, and the winter for 166. Astronomical facts, therefore, suggest that from time to time, during past ages, conditions have arisen in which the total heat of the year was imparted to the hemisphere much more equably than is at present the case.

A few moments' consideration will show what a beneficent climate is indicated for such happy epochs. In our temperate latitudes there is now a certain amount of severe cold every winter, and a very intense degree of cold once or twice in each decade, which is fatal to organisms of the more delicate description. There are numbers of semi-tropical plants which could thrive out of doors in Great Britain if the heat in the course of the year were distributed a little more equably. It is not that the total heat is insufficient, but a scarcity in winter is the necessary consequence of a redundancy during the summer. If only some of that redundancy could be systematically withheld during the summer months and kept over to mitigate the rigours of the ensuing winter, the flora of the temperate regions would experience a marvellous change. What is equivalent to this has undoubtedly taken place at certain epochs in the past. When such a condition of things has been brought about, then it endures for a period of many thousands of years before it gradually becomes transformed into a less favourable state. During all those ages the vegetation has had time to spread its luxuriance into solitudes where, from the rigours of our present climate, only a scanty vegetation now sustains a precarious life. Thus, even Greenland was once, or perhaps more than once, the abode of a beautiful flora which ornamented regions where now the severity of the climate is proverbial.

We thus see that the application of Wiener's theorem to the known laws of the variation of the Earth's orbit arising from the attraction of the other planets, is adequate to explain those phases of the Earth's history in which a genial climate overspreads a great part of one or the other hemisphere. By an application of the same principles we are now able to throw the required light upon the cause of the Ice Age. The elements of change in our system are such that from time to time the most varied dispositions in the seasons may present themselves. One combination of conditions explains, as we have seen, the occurrence of a genial period; another will account, in an equally satisfactory manner, for the phenomenon of glaciation. For in the lapse of time it has certainly happened once, or we might probably say more than once, that during a period of many centuries the northern hemisphere has, in each successive year, had a brief summer of 166 days and a long winter of 199 days. During such epochs the disparity of the seasons is at its greatest, for here again comes in the operation of Wiener's inexorable law. This declares that even when the winter is a month longer than the summer, there can be no relaxation of the principle which requires that 63 per cent. of the total Sunbeams of the year come upon the hemisphere during the summer, and that only 37 per cent. can be reserved for the winter.

Nor is it hard to see what the climatic consequences of such a dispensation must necessarily be. Have we not already pointed out that the present condition of the Earth produces a more rigorous climate in the temperate regions than has sometimes prevailed in the past, when the supply of heat was better adapted to neutralise the extremes of the seasons? We have now to demonstrate that the disparity between our present seasons and those exceptionally genial ones was balanced by a disparity in the opposite direction between our present seasons and certain others compared with which our climates must be regarded as themselves excessively mild. For suppose that the whole sixty-three units of heat were poured in on the Earth in 166 days, the summer would then be an extremely brief and an extremely hot one. Look at the figure on page 307, in which I have represented this condition of affairs in an orbit drawn accurately as to eccentricity. As the Earth moves round through P A Q the northern hemisphere enjoys summer, but the characteristics of that summer are of an exceptional type. It is brief, and it is extremely hot, for during midsummer, when the Earth is at A, the distance between the Earth and the Sun is actually at its smallest. If our globe were provided with any adequate means for storing the exceptional fervour which is so copiously dispensed to it during this fierce season, then, no doubt, the rigours of the succeeding winter might be mitigated by drawing on this store. It is true, as will be presently seen, that the Earth does fortunately possess, to some extent, a capacity for the temporary absorption of redundant Sun-heat. It follows from this that the actual climates of our globe are much less accentuated in their contrasts than they would otherwise be.

But though the horrors of an Ice Age may be lessened by this influence, they cannot be averted, for after passing the point Q, the Earth then enters on the long and dreary part of its track P B Q; then is the season of winter; and what are the characteristics of that winter? It is, as we have pointed out, extremely long. At midwinter the Earth is in the position B; it is then, as will be seen, at its maximum distance from the Sun. The cold to which our globe is exposed is then the greatest it can possibly experience under any combination of circumstances. And as if to increase to the uttermost the bitterness of this dread midwinter, the Earth's progress through this remote part of its orbit is made at the very slowest pace with which it ever moves. At length, however, the midwinter is past, and the globe gradually draws in again towards the Sun, accelerating its pace as it does so. Again it performs the comparatively rapid sweep around the Sun through the arc P B Q, with the accompaniment of a fiercely hot but brief summer, and then again it enters on a long and appalling winter. Doubtless, if such a succession of seasons were to be experienced for only a year or two, they would hardly be considered to provide an adequate explanation of a glacial period. But in its response to the attractions of the disturbing planets, the orbit of the Earth does not hastily alter its form. It can be demonstrated that periods to be estimated in hundreds of thousands of years are required for the waxing and waning of the elliptic path within the narrow limits through which it fluctuates. Hence it is that when such a condition of climate as I have last supposed has been established, it does not pass speedily away.

For many thousands of years each brief and intensely hot summer is followed by a long and bitterly cold winter, during which there is a steady accumulation of ice and snow which the succeeding summer is not able to dissipate. A large part of the Sun-heat which the Earth then receives is directly reflected back from the surface into space, hence it is that year by year the snows of the Arctic region increase. Year by year the limit of the snow-field creeps steadily southwards, until at last the temperate regions are largely invaded by an ice-sheet, and what we have described as a glacial period is the result. But all things have an end; the inequality of the seasons begins at last to decline. As they approach to equality the contrast between the seasons begins to be less acutely marked; the summer gets a little longer, the winter gets a little shorter; less snow can be made in one season, more snow can be melted in another. In the course of time it is found that the advance of the ice is checked, but still the progress of amelioration goes on, so that the ice is forced backwards, until at last it regains its normal limits in the Arctic regions and the Ice Age is at an end.

Some doubt may be felt as to the adequacy of the causes I have indicated to explain phenomena on such a gigantic scale as those which are involved in the advent of an Ice Age. I must therefore add a few words in elucidation of this point. I have already stated how important it is for us to realise that the radiation of the Sun maintains our Earth at a temperature at least 300° higher than that to which it would be reduced if the Sun were absent. Bearing this in mind, it surely makes an enormous difference whether 63 per cent. of the total Sun-heat bestowed upon the Earth be poured in in 199 days, and 37 per cent. in 166 days; or whether, in the opposite case, 63 units of heat be rapidly delivered in the comparatively brief period of 166 days, while 37 per cent. have to be eked out to cover so long a period as 199 days. In fact, in many ways the real matter for surprise is that the difference in temperature between summer and winter should be so small as it is actually found to be at the present moment. In some parts of Siberia the difference between the highest temperature of summer and the lowest temperature of winter amounts to not less than 150°. Judging by our ordinary temperate climates, this no doubt seems a stupendous difference; but a little consideration of Wiener's law will show that even so marked a contrast is not to be wondered at. In fact, it is hardly too much to assert that a divergence between the two seasons somewhat like that found in Siberia would be universal throughout a large part of the northern hemisphere, were it not for the general operation of mitigating causes, from the benign influence of which those dismal Siberian regions are unhappily withdrawn.

I shall endeavour to make this plain by an illustration, and instead of the numbers 63 and 37, so important in Wiener's law, I shall, for the convenience of the moment, take the numbers 5 and 3, the proportion of which is nearly the same. We shall attempt an approximate calculation—it can only be an approximate one—of what the divergence between mean summer and mean winter temperature might average throughout the hemisphere if there were not equalising agents which stored up heat in summer and issued it in winter. Of course, I am aware that the illustration now given can only apply imperfectly to the facts of nature. But it will, I think, suffice for our object. For the sake of rendering the matter clear, I may put the argument in the following form. Suppose that a hot-air chamber could be continuously maintained at a temperature 300° above that prevailing outside, by a regular consumption of coal in suitable furnaces at the rate of eight tons per annum. Let us now suppose that, instead of burning at the uniform rate of two-thirds of a ton per month, five tons of coal were to be consumed uniformly in one half of the year, and only three tons in the other, what would be the contrast between the temperatures maintained in the chamber in the two seasons? Ordinarily speaking, four tons of coal would be consumed during the six months to obtain the desired elevation of the heat in the chamber over that outside. If, however, five tons of coal were consumed during the same period, it seems not unreasonable to assume that the exaltation of temperature thus obtained would be proportionate to the increased consumption—that is to say, the temperature of the chamber would be kept during the six months at 375°. In the succeeding six months, when only three tons of coal were allowed instead of four, which we have supposed to be necessary for keeping up the standard temperature, it might be expected that a thermometer in the chamber would fall 75° below the standard—in other words, it would only indicate 225° above the environing air. We should therefore find a contrast between the temperatures maintained in the chamber during the two halves of the year amounting to the difference between 375° and 225°—that is, to 150°. This illustration will apply to the case of the northern hemisphere regarded as the chamber, and the heat of the Sun as the fuel. Five-eighths of that fuel are consumed during one half of the year, and three-eighths during the other half. We know that the average heating of our Earth for the whole year is sufficient to maintain it at 300° above the environment. Need we therefore wonder that the actual difference between the greatest temperature of the hemisphere in one six months and the least temperature in the other should attain the value it has actually been observed to reach in the interior of Siberia?

But in the illustration just given, I have supposed the seasons to be of equal duration. Let us now consider the case when they differ in length to the greatest extent possible. Let us suppose that eight tons of coal per annum were to be devoted as before to the maintenance of heat in a chamber, and let us estimate the temperatures which could be kept up if five tons of coal were to be expended in 166 days, and three tons in 199 days. A simple calculation on the assumption that the temperature maintained above the environment is proportional to the fuel consumed, will show that the temperature of the chamber above the environment during one of the seasons will be as high as 412°, and during the other only 206°. We see that there is a difference of not less than 206° between the two cases.

From this reasoning it seems at first astonishing that the contrast between summer and winter in our temperate localities is not more strongly accentuated than we actually find it to be. It is the presence of the mitigating agents to which I have referred which explains the difficulty. Of course, the waters of the ocean form the chief equaliser. No doubt the temperature of the water in the open sea fluctuates to some extent between summer and winter, but not at all in proportion to the actual variation in the receipt of heat from the Sun in the corresponding seasons. The ocean-covered part of the Earth's surface thus forms a vast reservoir in which the redundant heat of summer is largely stored up, so that it may be doled out a little later to mitigate the rigours of the ensuing winter. Hence it is that insular climates enjoy, generally speaking, an immunity from extremes of temperature. Hence, too, it is that certain regions happening to be excluded from the beneficent climatic influence of the proximity of the sea, exhibit these tremendous contrasts of summer and winter temperature which serve as illustrations of what would be the normal condition of our globe if mitigating agents were absent.

It now only remains to say a few words with respect to the laws by which the succession of Glacial and Genial periods on our globe has been regulated. It can be shown that the Earth's orbit, having once attained a high eccentricity, will generally retain it for a period of certainly not less than 100,000 years. Let us suppose that at the commencement of the period referred to the northern hemisphere was in the glacial condition. It follows from the principles already laid down that when the northern hemisphere is glaciated the southern must be enjoying that genial condition of things in which a luxuriant flora is permitted to extend its limits widely. This is an essential part of the astronomical doctrine of Ice Ages. The theory requires us to admit that a glacial state in either hemisphere must be concurrent with a genial state in the other. We must not, however, suppose that the climatic circumstances of the two hemispheres would remain unaltered for the whole 100,000 years during which the high eccentricity lasts. The position of the line of equinoxes gradually shifts on the orbit, so much so that in about 10,000 years it moves half-way round with regard to the axis of the ellipse. During this interval an interchange of climate between the two hemispheres has been completed, the long winter has gone to the southern hemisphere and the short one to the northern. Accordingly it is now the southern hemisphere which is glaciated, while the northern enjoys a genial climate. In 10,000 years more the line of equinoxes has completed its revolution and the former condition of climates will have been restored.

We thus see that while the eccentricity continues high, there will be an incessant oscillation of glacial conditions between the northern hemisphere and the southern. The occurrence of such alternations is a vital doctrine in the astronomical theory of an Ice Age. So long as the eccentricity of the Earth's orbit remains high enough to occasion a sufficient disparity between the lengths of the seasons, so long will icy conditions perform these oscillations between the two hemispheres. Here we must admit that a somewhat more numerous succession of Glacial and Genial periods is demanded by the astronomical theory than would seem to be demonstrated by actual geological observations. It is, however, I believe, admitted by most geologists that direct evidence is available to prove that there has been more than one Ice Age, if not several such events. It is also to be remarked that the agents in operation during each Ice Age would tend to remove the traces of the preceding Ice Ages, and that thus the geological record as now presented to us is necessarily of a very imperfect character. The astronomical theory asserts that during the course of geological time the conditions necessary for producing a high eccentricity in the Earth's orbit must from time to time have recurred. Whenever this state of the system was brought about, it seems certain that Ice Ages and Genial Ages must have alternated in each hemisphere until such time as the eccentricity slowly abated. We have thus a picture of a series of clusters of alternating Ice Ages and Genial Ages, each cluster being followed by the next after hundreds of thousands of years.

It is, indeed, impossible to doubt the truth of the main factors in the astronomical theory of the cause of Ice Ages; nothing can be more certain than that the planets disturb each other, and that in consequence of these disturbances our Earth's orbit waxes and wanes in the manner we have described. Calculations of these changes are made by the application of certain mathematical principles which have never been found wanting when an opportunity has arisen for testing the interpretations of astronomical phenomena based upon them. It is therefore certain from astronomical causes alone that there must have been remarkable climatic vicissitudes during past ages. It is reasonable to suppose that the effects of those vicissitudes are to be observed in the marks and monuments by which geologists have demonstrated from their point of view the former existence of Glacial and Genial periods.

CHAPTER XVII

THE SUN AS A STAR.

BY our residence on the Earth we are somewhat disqualified from observing directly the true position of the Sun in the scheme of things celestial. It is necessary to imagine a change in the point of view from which we regard the universe before we perceive in its proper proportions the relation which the Sun bears to the surrounding objects. If we were able to wing our way from this Earth into the depths of space, and if in the course of our journey we looked back from time to time on the system we had quitted, we should presently find that the Earth became more and more insignificant, until at last it was quite invisible; the small planets first, and the larger planets afterwards, would drop entirely out of view. If the voyage continued for hundreds and thousands of millions of miles, the Sun would begin to be shorn of those supereminent splendours which at present distinguish it so conspicuously from all other objects in the sky. When at last the traveller had gone far enough to obtain a convenient view of the neighbouring stars, the Sun would no longer appear to occupy an exceptional position; it would have sunk to the insignificance of a small star, not nearly so bright as many of those which adorn the heavens every night. In fact, the cardinal truth of modern astronomy is that which teaches us the modest position of the Sun in the celestial scheme; we learn that our orb is, after all, no more than a star, and not by any means an exceptionally bright star. To us, of course, this particular star appears with a brilliance and an importance wholly exceptional; but that is simply because we happen to be nestled up comparatively close to it for the enjoyment of the light and heat that it dispenses so liberally, while we are separated by quite appalling distances from the other stars. Were our Sun as distant as most other stars, it would be barely discernible.

Probably many of the uncounted myriad of stars scattered through space may themselves be attended by systems of worlds revolving around them; but it is wholly impossible from our position to see anything of such dark worlds. All that we can discern from this distance are the incandescent points by which, if those other worlds exist, they are illuminated. No doubt we are enabled by indirect processes to become conscious of the existence here and there of dark globes in the immediate vicinity of certain of the bright ones. The eclipses of the famous star Algol, for instance, have been recently shown to be attributable to the interposition of a dark star, at regular intervals, between the observer and the brilliant point. But we cannot, under any circumstances, see this dark star itself, and we could never have learned of its existence except that it obscures another star which we can bring under observation.

The theory of probabilities compels us to believe that the bright suns must be outnumbered in the proportion of hundreds or thousands, or even of millions to one, by dark invisible stars. We must remember that the incandescence by which a mass of celestial matter is rendered visible from afar must be regarded as a somewhat exceptional incident in the history of the body thus affected. The periods during which any individual mass of matter glows with such radiance must, on the average, be almost infinitesimal when compared with the prodigious intervals during which the body is dark and incapable of radiation. It is thus reasonable to believe that the millions of stars which we actually see are simply the conspicuous members of a vastly more numerous host, of whose existence we are almost entirely ignorant save for such glimpses of knowledge as the theory of probabilities can afford. As the Sun is a star, it becomes of much interest to consider the nature of that star, and to inquire in what respect it resembles other stars. We have discussed the question in another chapter as regards the movement of the Sun through space; we have now to consider it as regards the individual peculiarities of the different stars.

Seeing that we generally find stars to be arranged in those groups which we call constellations, it would be interesting to learn how far the Sun may be assigned to any group of stars that can be so designated. It would seem, so far as our present knowledge goes, that the answer to this query must be negative. It does not appear that there is any particular group of stars which could be specially singled out as associated with the Sun in the celestial scheme. It seems clear, for instance, that no stars could be pointed out which bear such a close affinity to our particular star as Alcyone bears to Merope and the other members of the Pleiades. Nor does it seem likely that the Sun belongs to any constellation with whose members it forms an alliance at all resembling that which unites the three stars in the belt of Orion with Rigel and Betelgeuze into one glorious association. Spectroscopic evidence seems to show without any doubt that in the case of the principal stars in Orion their apparent contiguity in the heavens is connected with a veritable community of organisation. I do not, however, see any particular reason to think that the Sun belongs to any group which can be regarded as analogous to the constellations I have mentioned.

It would seem, however, that the Sun does belong to one assemblage of stars, which, so far as its dimensions are concerned, is of stupendous grandeur. The Milky Way is well known to be constituted by multitudes of minute stars, the smaller of which tax the powers of a good telescope to reveal them. These stars are so distributed as to form a zone of light which, with more or less irregularity, encircles the heavens. It is also to be noted that the track of the Milky Way, or the Galaxy, as it is often called, is very nearly that of a great circle of the celestial sphere, and consequently the solar system must lie within the boundary of the Milky Way. This will be obvious from the fact that if we were placed anywhere outside this cluster it might, indeed, appear to occupy a circular region of the sky, but that region would not take the form of a great circle, as it actually does. The fact that the Milky Way is a great circle is demonstrative of the fundamental truth that the Sun lies in this mighty stratum of stars.

Some examination of the construction of the Galaxy is therefore desirable before we attempt to assign the true rank of our Sun in the universe. The great difficulty in the comprehension of the structure of the Milky Way is the same that we meet with in almost all branches of sidereal astronomy. It arises from our general ignorance of the distances by which we are separated from the objects at which we are looking. We can hardly be said to have any knowledge as to the distance of the minuter points of the Milky Way, except what may be gained by judicious surmise. No doubt the delicate parts of the Milky Way, where it merely seems like a light stain on the sky, are composed of excessively remote stars. As we are surrounded by the zone of stars, it seems impossible not to believe that the collection of stellar points which make up the Galaxy must have a shape somewhat resembling a flat disc, near the centre of which the solar system is situated. The stars on the outer part of the disc are those which produce the faint nebulous light; the more central parts of the disc contain the stars which lie nearer to us, and are therefore, on the whole, much brighter. The question then arises as to whether many of the ordinary lucid stars which we see in the heavens may not be merely some of those objects in the Galaxy which lie, comparatively speaking, in our neighbourhood.

The theory of probabilities applies here, as elsewhere in astronomy. By counting the number of bright stars in the sky it is easy to calculate the probable number that should be found on any given area, supposing that they were distributed quite at random over the heavens. Now it is a remarkable fact that the zone occupied by the Milky Way not only displays the luminosity characteristic of the Galaxy, but also contains within its limits a far larger proportion of bright stars than would naturally belong to an area of equal extent, if the stars were equably distributed. The laws of probability therefore assure us that the stars which we find so copiously strewn along the Galaxy have some physical association therewith. It is, then, unreasonable to doubt that many of them really belong to the Galaxy—in other words, it seems certain that many of the bright stars strewn on and about the margin of the Milky Way actually belong to that cluster. As to the precise disposition of the stars forming the Galaxy, there is considerable difference of opinion. It seems, however, perfectly certain that whatever may be the shape of the actual structure, whether it have the form of a disc, as in the theory which seems to me the most reasonable, or whether it take the form of a stream of stars with occasional serpentine folds, or whether, again, the stars be disposed in a gigantic ring like those exhibited by the wonderful spiral nebulæ, it is, at all events, certain that the Galaxy forms an organised cluster in space, and that the Sun is in that cluster.

Astronomers are well aware that the heavens contain multitudes of other star-clusters which are often objects of remarkable shape. They present, indeed, some of the most striking among sidereal pictures. In parts of space, however, quite remote from that occupied by the Galaxy, other clusters can not unfrequently be discerned; and astronomers have boldly conjectured, with some plausibility, that such clusters, which are often so faint as to tax the powers of the mightiest telescope, may be in truth galaxies of a different order. It may, indeed, be that these clusters consist of suns perhaps as numerous and perhaps as individually magnificent as those which constitute the Galaxy in which we reside; but owing to the tremendous depths of space in which they are plunged, we are unable to distinguish more than the collective light distributed by them. It has thus been suggested that there are in space orders and sub-orders of organised groups of stars; so that, just as the Earth is one member of the group of bodies which revolve around the Sun, so the Sun itself is one member of the mighty host of stars which form the Galaxy, while the Galaxy is not improbably only one cluster out of many clusters strewn through space. A photograph by Dr. Isaac Roberts of the cluster known as M 37 in the constellation of Auriga is here represented (Fig. 75). It shows how numerous are the stars in such an object. The exposure given was ninety minutes; and it is to be observed that with each increase in the exposure the number of visible stars is found to augment. Indeed, it seems certain that on many of our plates stars can be discerned which are far too faint to be ever perceived by the telescope.

Among the other constellations of stars with definite bonds

FIG. 75.—PHOTOGRAPH OF CLUSTER IN AURIGA. (*Dr. Roberts.*)

of association, we may mention the well-known group formed by Ursa Major. There are ample grounds for believing that the seven famous stars forming the Plough are associated with each other, inasmuch as five of them are animated by movement in a common direction. It would be almost inconceivable that five stars should be moving in the manner in which these actually do move, if there were not some physical explanation of the uniformity. But we cannot assign any physical reason unless we admit that there is some association between the stars of the Plough besides that arising from the casual circumstance that they happen to lie in the same vicinity on the heavens.

A case of kindred significance is found in the group known as Cassiopeia. This constellation exhibits an arrangement of stars forming a remarkable configuration. When it is further perceived that these stars partake of a common motion, we cannot refuse to entertain the belief that the apparent association is a real one. It has often been noticed that there is a tendency among the fainter stars to be disposed in streams. Indeed, many of the brighter stars in the constellations exhibit the same feature. For instance, look at the remarkable arrangement displayed in Corona Borealis. The adjustment of stars in this way is wholly inexplicable by mere chance distribution of stars on the sky. But the most striking instance of the disposition of bright stars in the form of a stream is seen in the well-known case of the constellation Eridanus; indeed, the very name of this group, which is that of a semi-mythical river, shows that the stream-like arrangement of the stars had attracted the attention of the ancients.

Star distribution is well illustrated in the adjoining photograph, taken by Professor Barnard, at the Lick Observatory, on 8th November, 1892. It derives special interest from the fact that the central object in the picture is Holmes' Comet. The great Nebula in Andromeda is shown at the top, on the left-hand side. Here, as elsewhere over the sky, the picture is studded with myriads of points, each of which is a star.

Owing to the stupendous distance of the stars, our efforts to compare them with the Sun are limited by the very scanty knowledge available as to their actual characteristics. One group of primary facts relative to their colour can, however, be considered. So far as its hue is concerned, the Sun occupies an intermediate position. A little illustration will render this plain. We have, first of all, a number of brilliant gems, of which Sirius and Vega may be taken as typical, the light from which is distinctly whiter

Fig. 76.—Holmes' Comet, and the Great Nebula in Andromeda, November 8th, 1892; Exposure Three Hours.
(*Barnard.*)

than that radiated by the Sun. Capella has a somewhat more yellowish colour than the two bright stars we have named, and it would seem that the light which it diffuses has much the same hue as that of the Sun. We find a distinctly reddish tint in the radiations of the star Arcturus. Still more remarkable are the colours of Aldebaran and Betelgeuze, which stars may, in fact, be described as actually ruddy. The reddest of all the stars visible to the unaided eye is the famous garnet star of William Herschel, in Cepheus. It requires, however, some slight telescopic assistance to render the remarkable hue which it possesses fully appreciable. Among the smaller telescopic stars, some very remarkable red objects are to be noticed. One of the most notable of these red stars is that known as R Leporis, discovered by Mr. Hind, who describes it as a blood-drop on the background of the sky. Many other similar objects more or less remarkable for their colours are described in the excellent catalogue prepared by the late Mr. Birmingham, and added to and extended by Mr. Espin. It should, however, be remarked that those who have the use of a telescope are often disappointed by finding that the colours which the stars actually display are not quite equal in splendour to those which their imaginations had pictured, from the descriptions of stellar hues in astronomical text-books. No doubt eyes differ to a considerable extent in their capacity for the appreciation of colour. It must, however, be admitted that considerable training is required for the proper estimation of the degree in which the colour of any particular star is remarkable. It need not therefore surprise us if an astronomer, who has examined thousands of stars, describes in emphatic language the colour of one which he finds more intense than any of the rest. How else could a due sense of proportion between the appearance of this star and that of the others be preserved?

The most singular circumstance connected with the colours of the stars appears to be the non-existence of any isolated star of a distinct greenish or bluish colour. To some extent this circumstance may be imputed to the tax which our atmosphere levies on the blue rays of light traversing it. The ruddy glows at sunset show that the red rays are transmitted with comparative ease through the atmosphere, while the bluish end of the spectrum is absorbed by the vast atmospheric thickness through which the rays of the setting Sun have to pass. Even when the Sun has a considerable altitude there is a strong preferential absorption of the rays at the blue end. The consequence is that if we were able to view the celestial objects without the intervention of our atmosphere, they would all transmit to us a light which had far more blue in its composition than that which now reaches us. Indeed, as Professor Langley declares, the Sun itself, though so white to us now, would seem actually blue if we could see it without the interposition of our atmosphere or its own. In like manner, Sirius and Vega, instead of shining with their actual intense whiteness, would probably appear distinctly blue. The more ruddy stars, like Aldebaran or Betelgeuze, might indeed appear almost white; and probably only stars of an extremely red type would retain their hue if presented to the eye of an astronomer freed from the disadvantage of being able to examine only rays which have suffered from a vast atmospheric journey.

This consideration goes far to explain the marked absence from the sidereal host of distinctly blue objects; not only are there no brightly blue or green stars visible to the unaided eye, but even the telescope fails to reveal any such stars existing isolated in the heavens. It should be explained why it has been necessary to qualify this statement by the word "isolated." It is a singular circumstance that although a certain number of bluish or greenish stars are well-known telescopic objects, each such object is one of a close pair, forming a double or a binary star; while, generally speaking, the other star of the pair has a hue which is also of a striking type. The most celebrated object of this description, and, indeed, one of the most lovely stars in the universe, is that known to every possessor of a telescope as Beta Cygni. We have here a pair of suns associated together, the one described as possessing the hue of a topaz, the other that of an emerald. It is an exquisite object, and for a long time it was thought that the sharply contrasted hues presented by the two stars, forming as they do complementary colours, might possibly be due to some optical illusion.

It seems hard to believe that something of this kind may not be the case. I recollect one fine morning, many years ago, that as I was walking through a shrubbery at Parsonstown, my attention was attracted to some dew-drops hanging to a spider's web, on which the Sun was shining brightly. I noticed the vivid hues transmitted by the little beads of water, and after some trial I succeeded in so placing my eye that the refracted sunlight from two of the water-drops displayed exactly similar hues to those which I had been watching with much delight in Beta Cygni on the previous night through the great telescope at Parsonstown. I do not for a moment mean to assert that this little observation suggests any clue to the explanation of the beautiful colours of this renowned double star. I only say that it seems to make some optical explanation possible; for if certain stars are really of such a hue as that shown in the small component of Beta Cygni, how comes it that they are never to be found save as members of multiple combinations? It is difficult to see what particular physical conditions can be imposed by the association of two stars which not unfrequently tend to develop hues otherwise unknown in one or both members of the pair. I say one or both of the members, because it is well known that in certain cases we find a double star whereof both the components exhibit a greenish or a bluish tint. There is, for instance, the exquisite star Gamma Andromedæ, in which a large ruddy star is associated with a small greenish-blue one. By the application of sufficient optical power, the greenish-blue star itself divides into two separate points, each of them being of the hue so unknown in any isolated stellar object.

It must, however, be remembered that the celebrated observations of Sir W. Huggins on the spectrum of Beta Cygni appear to place it beyond doubt that some specific absorption takes place in the spectrum whereby the characteristic blue colour is given to the smaller star. Sir W. Huggins finds that the blue end of the spectrum in the larger of the two stars is subdued by so many bands as to leave the ruddy end of the spectrum predominant. On the other hand, there are multitudes of absorbent bands at the red end of the spectrum of the smaller star, the effect being, of course, to permit only the transmission of chiefly blue light. As to what may be the cause of the absorption by which these characteristics are imparted to the spectrum, we have at present no information.

In connection with this star it should be noted that up to the present no sufficient research has been made upon it with the view of determining its parallax. Doubtless ere long a systematic attempt to solve this interesting problem will be made; in fact, it appears from the measurements recently effected by Jacoby on photographs taken by Rutherford of stars in the vicinity of Beta Cygni, that the parallax of this star is not only of a recognisable magnitude, but seems from the indications so far given to be considerably larger than that of any other known star in the northern hemisphere. It would be indeed interesting if the exquisite double should turn out to be one of the nearest of the Sun's neighbours.

Secchi was the first to make an elaborate study of the various classes into which the stars may be divided, as regards their physical condition. The basis of the classification is provided by the appearance and disposition of the lines in the stellar spectra. The first class consists of stars whose spectra are characterised by a small number of thick absorption bands. Of these stars Vega is the most notable example. The second class comprises those stars in which the spectrum is crossed over by a vast multitude of fine and clearly marked lines. Capella may be taken as a striking instance of this class, to which, it should be added, the Sun apparently belongs. The next two classes have this much in common, that in each case the brightness of the light in the spectrum is largely subdued by extensive bands of absorption. Such bands are often beautifully graded and fluted, and give to the spectrum a marvellously

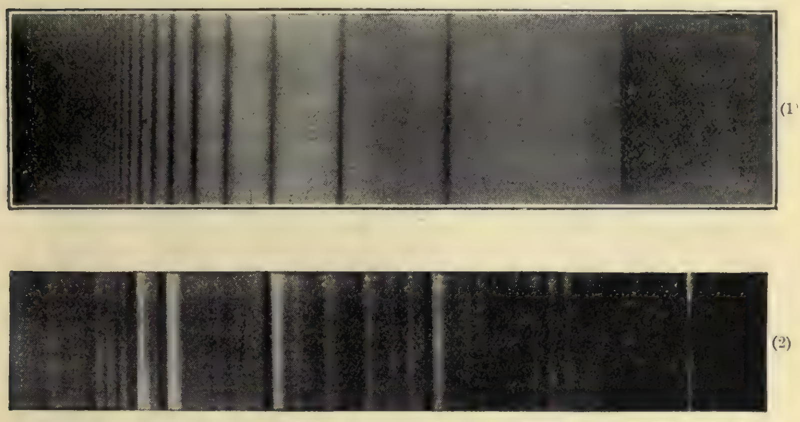


Fig. 77.—Spectrum of (1) Alpha Lyræ (Vega); and (2) Nova Aurigæ.

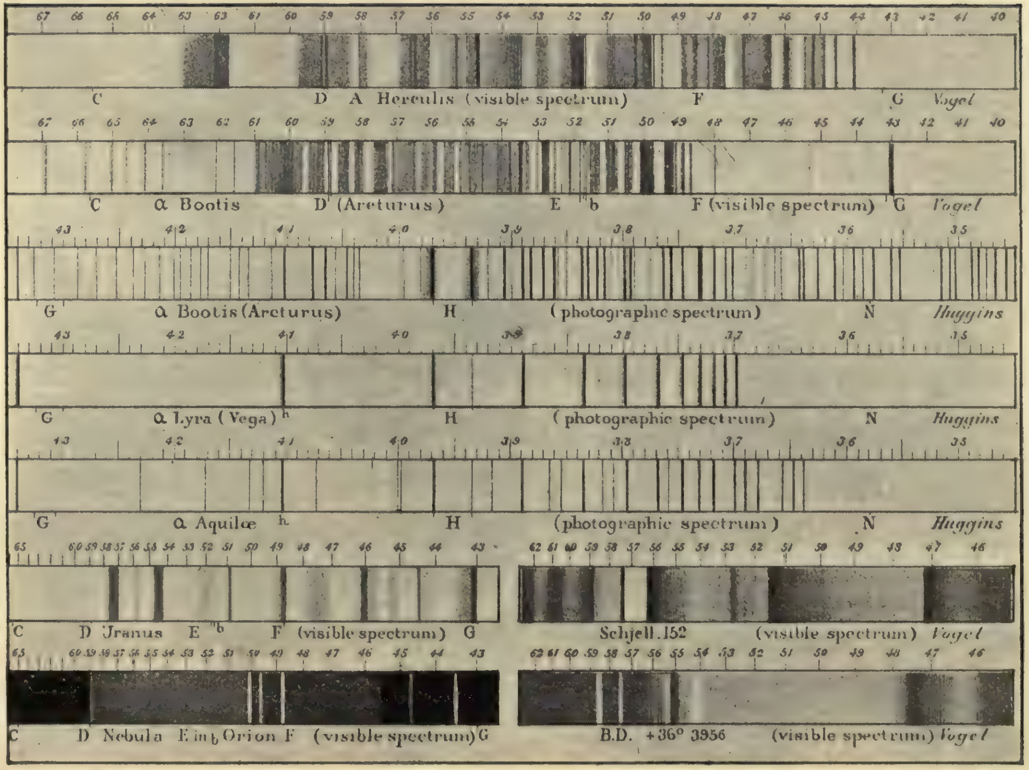


FIG. 78.—SPECTRA OF STARS.

interesting appearance. But there is this difference between the third and fourth classes: in the former, the flutings are sharply terminated towards the violet, but diffuse towards the red end of the coloured band of light, while in the latter, the arrangement is exactly reversed. As an example of the fourth class, we may cite a star well known to astronomers by the designation 152 Schj., this being the number which the star bears in the valuable catalogue of red stars which was compiled by the astronomer Schjellerup. The third class, where the flutings shade from the blue towards the red end, may be typified by the lucida of the constellation of Orion—the star Betelgeuze, to which we have already referred. The fifth class is one of a very remarkable character, including, however, but a comparatively small number of stars. The spectrum of a star belonging to this order, which, as in all other cases, is crossed by dark lines more or less numerous, presents an additional feature. Besides the dark lines, certain bright lines are present. This character, generally speaking, appears to be connected with some variability in the light of the star, and seems to point to the presence of masses of incandescent gas at a level considerably above that from which the bulk of the light emanates. As a specimen of this fifth type we may mention Beta Lyræ, the second brightest star in the constellation of Lyra. This classification of stars by their spectra undoubtedly corresponds with marked physical peculiarities in the several groups of objects.

Mr. Lockyer, in his theory, arranges the stars in seven groups, according as they are of ascending or descending temperature. Those stars of a brilliant white, like Alpha Lyræ in the first class, are the hottest of all objects; while, according to this view, Beta Lyræ, being one of the fifth class, is among the coolest of the lucid stars. On this supposition it will be noted that the Sun must be ranked not among the very hottest stars, but in the second class, so far as fervour is concerned.

As an illustration of the spectrum of a star of the first order, Fig. 79 may be taken. It is copied from a photograph taken by Professor Vogel, and shows the spectrum of Sirius and certain of the principal iron lines.

Among the more recent researches on the distances of the stars, I may mention one that is specially important to our present purpose, inasmuch as it illustrates in some degree the dimensions and the splendour of one of the other suns in space. Dr. Elkin, in pursuance of an elaborate series of researches on the parallaxes of celestial objects, has determined the distances of the following ten stars of the first magnitude:—Aldebaran, Capella, Betelgeuze, Procyon, Pollux, Regulus, Arcturus, Vega, Altair, and Alpha Cygni. These measurements seem to have been made with extreme care, the instrument employed being the heliometer of six inches aperture at the Yale Observatory. Taking them as a whole, their result has been to demonstrate that the distance of first magnitude stars is even greater than we had previously been led to expect.

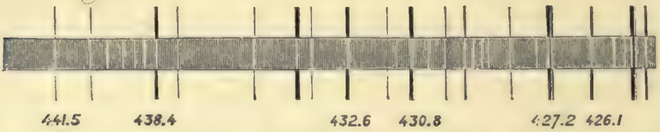


Fig. 79.—Spectrum of Sirius compared with Iron Lines.

The nearest of these objects is Procyon, "the little dog." Its parallax, as determined by Elkin's researches, is 0·266 of a second; amplifying this statement, it means that the distance of Procyon is such that an observer stationed at that distance would find that the radius of the Earth's orbit subtended an angle which was only a little more than a quarter of a second. This statement, however, hardly conveys an adequate idea of the actual distance at which this object lies. Perhaps it will be better realised by calculating how large the Earth's orbit would appear to an observer who was stationed on Procyon. It would seem to him about the same size as a penny-piece would look to one who viewed it from a distance of eight miles. This statement gives some idea of the delicacy required in operations for determining stellar distances. The investigation is concerned with the shift in the apparent place of the star when the observer alters his position from one side of the Earth's orbit to the other. Imagine the difficulty of a Liliputian seeking to measure the distance of an object some eight miles away by observations at the end of a base-line little more than an inch long. Of the same order are the difficulties of the astronomer.

According to the results of Dr. Elkin, other stars on his list are more distant than Procyon. Aldebaran, for instance, must be more than twice as far off; while Vega must now, it appears, be translated to a distance considerably greater than was hitherto supposed, Dr. Elkin's work making it seven times as remote as Procyon. But the most remarkable feature in the list is undoubtedly the extreme distance at which Arcturus would now seem to be located. The parallax which has been assigned to it by the work at Yale is sixteen thousandths of a second. Of course it is not possible to claim any extreme accuracy in a research of this description, but it certainly seems that Arcturus must be at least ten or fifteen times more distant than Procyon. It appears hardly possible to doubt that an observer on Arcturus, looking towards this Earth, would perceive its orbit round the Sun as no larger than a penny-piece would seem to one who viewed it from a distance of 100 miles. So astonishing an assertion naturally challenges very rigorous scrutiny. Dr. Elkin's well-known reputation as a skilful observer is sufficient guarantee for the reliability of the result. He entertains no doubt whatever that the parallax of Arcturus must be as minute as these figures imply. The value that he has assigned is based upon eighty-nine observations; nor are these dependent on a solitary star for comparison. Dr. Elkin has employed five pairs of stars for the purpose, and there is satisfactory concordance between the several measurements.

The chief reason why astronomers have been so much struck at the assignment of so great a distance to Arcturus, depends on the fact that it is one of those stars which has a considerable proper motion. Each year Arcturus shifts its place on the sky by an amount represented by 2·3 seconds of arc —that is to say, in each year the position of Arcturus has changed from the place which the star occupied in the previous year by the figure we have named. Of course, as an angular magnitude this is a very small quantity indeed—it is not the hundred-thousandth part of a right angle; but compared with the movement of the great majority of the other bright stars, the proper motion of Arcturus is by no means small. It is, indeed, exceptionally large. I do not mean that there are not other stars which apparently move quite as fast as Arcturus; there are, in fact, some which have a proper motion twice or three times as great; and there is one, the famous star Groombridge, No. 1830, which moves more than three times as quickly. But these are all small stars; it is among the bright stars that Arcturus is remarkable for its proper motion.

We have recently learnt in a different way that the proper motion of this star is exceptional. I have mentioned in another chapter the important principle of the method by which the spectroscope has enabled us to determine the movements of celestial bodies along the line of sight. Professor Vogel and Dr. Scheiner have made classical researches on this subject at Potsdam; and such is the precision which they have obtained that, as Prof. Vogel declares, the probable error of the results may be estimated at less than a single mile per second. The mean of the velocity along the line of sight thus determined for fifty-one of the brightest stars was 10·4 miles per second; six stars were found with a velocity of less than two miles a second, and five had a velocity greater than twenty miles a second. The list is headed by Aldebaran, which seems to travel at a pace of not less than thirty miles a second. By the application of the same spectroscopic method, Arcturus was found to have a speed of about five miles per second along the line of vision.

We have already had occasion to point out that the element of proper motion indicated by spectroscopic research is wholly distinct from that element which is indicated by the apparent displacement of the star over the surface of the sky; in fact, the true proper motion of the star is only to be ascertained by compounding the radial proper motion, as given by the spectroscope, with that on the surface of the heavens, as given by the telescope, and deducing the resultant of the two. There is, however, a characteristic difficulty in the application of the process; for whereas the spectroscopic method gives us the velocity in miles per second, the telescopic method only expresses the velocity in seconds of arc, and before this can be transformed into miles per second, the distance of the object has to be brought into the calculation. Employing, however, Dr. Elkin's determination of the star's distance, it appears that the velocity of Arcturus, perpendicular to the line of sight, is about 380 miles a second. This has to be compounded with the velocity along the line of sight, as determined by Vogel, of five miles a second. It is therefore obvious that the movement of Arcturus is almost entirely perpendicular to the line of view. To combine the two we should, strictly speaking, add their squares together and take the square root, the result being, however, so nearly the same as the larger component that, so far as our present knowledge goes, Arcturus may be considered as hurrying along at a total pace of 380 miles a second. So terrific a speed seems to be unparalleled in the pace attained by any other celestial object. Compare it, for instance, with that of the star Groombridge, No. 1830, the parallax of which was carefully investigated by Brünnow. The proper motion of this rapidly moving object being also determined, it was easy to show that the velocity with which it was animated must be no less than 200 miles a second. This is, of course, only that component of the motion along the surface of the sky; to it would have to be added whatever might be due to radial velocity along the line of sight. This element has not, I believe, been determined for this star. However, it does not seem in the least likely that its total velocity can be equal to that which we must attribute to Arcturus.

As to the intrinsic brilliancy of Arcturus, we are now able to form some conception, for photometric researches have established the ratio of its apparent brightness to the apparent brightness of the Sun. From the figures thus available we are able to calculate what the brightness of our Sun would appear to be if it were removed from us to the same distance as that by which we are separated from Arcturus. It seems quite certain that in this case the Sun could not transmit even the one-hundredth part of the light which we receive from Arcturus. This is merely

Fig. 80.—PHOTOGRAPH OF NUBECULA MAJOR.

(*Taken by H. C. Russell, 17th October, 1890; Exposure, 7h. 3m.*)

equivalent to saying that the intrinsic brightness of Arcturus must be at least one hundred times the brightness of the Sun. It must, however, be noticed that these figures refer only to the question of lustre; they tell us nothing with regard to the real magnitude of Arcturus, and still less as to the weight of this star. Doubtless Arcturus does vastly transcend our Sun both in magnitude and in weight; but it is at present impossible to speculate in what degree, though, as we shall now see, we are provided in certain other instances with an effective means of weighing stars comparatively to the Sun.

I must here specially mention the famous object Mizar, the middle star in the tail of the Great Bear. This body has long been familiar to the astronomer; it is, in fact, known to every beginner in the science as one of the most interesting double stars which the heavens contain. Viewed with ordinary telescopic power, Mizar seems to split into two distinct suns, the little star Alcor, which can be discerned by the unaided eye in the immediate vicinity of Mizar, being removed to a considerable distance in the telescopic picture. Doubtless the various stars of the system are allied by some bond of union, though it must be admitted that up to the present no movement has been detected of such a character as to show that the stars are in mutual revolution. Recent research with the spectroscope has, however, demonstrated a very interesting circumstance with regard to the larger of the two stars forming the double. The spectrum presents, as stars usually do, a series of dark lines. These lines have been photographed, and when a sufficient number of photographs were obtained, it appeared that some of the lines were occasionally seen to be distinctly double. Subsequent examination showed that this doubling of the lines was a periodical phenomenon. It appeared that at definite intervals each of the lines referred to opened out into a pair and then closed up again, the movement being performed with such regularity that it seemed obvious there must be some physical reason for so marked a periodicity.

Our knowledge of the circumstances under which a spectrum is produced explains this phenomenon. It is obvious that when the lines are doubled, the light from whence the spectrum

FIG. 81.—PHOTOGRAPH OF NUBECULA MINOR.

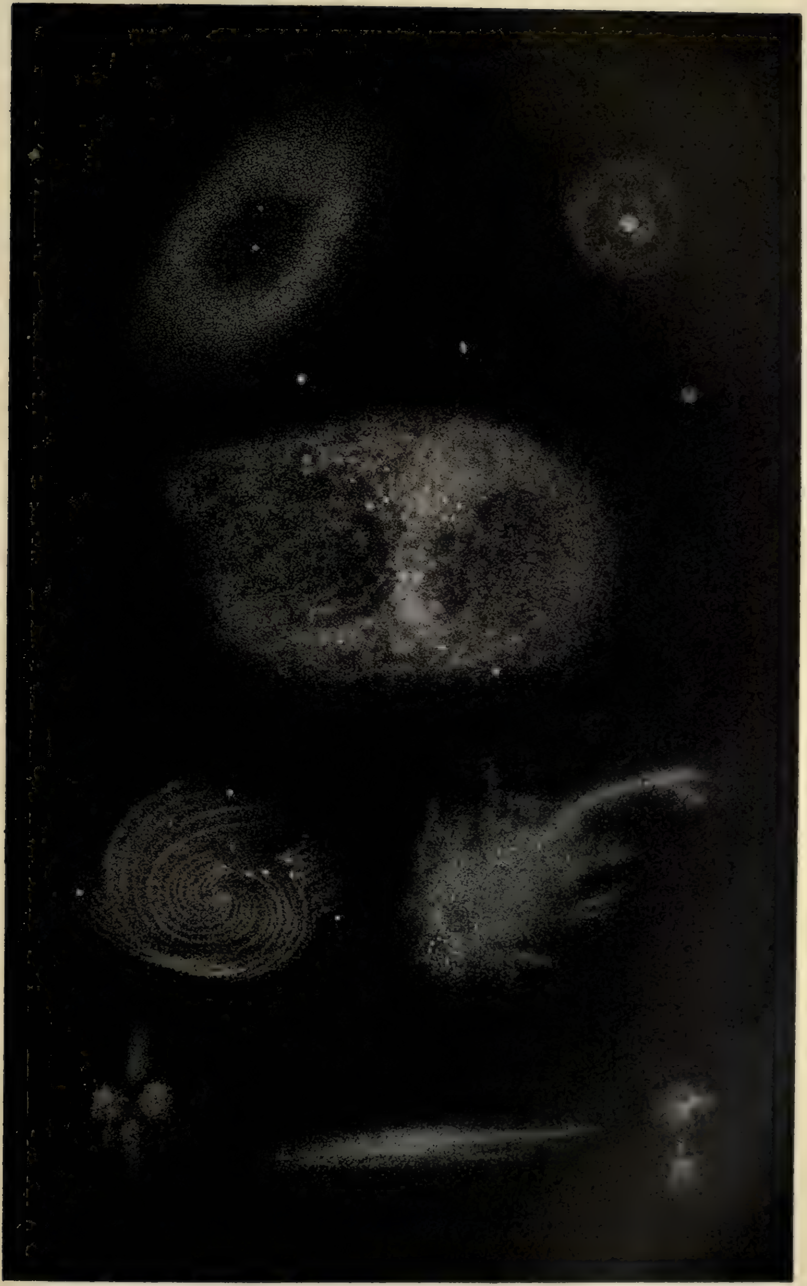
(Taken by H. C. Russell, 14th to 15th October, 1890; Exposure, 8h. 0m.)

originates must have a double source. The wave-lengths of the lines belonging to one element, say hydrogen, have different refrangibilities, when emitted from one of the sources, from what they have when emitted from the other. The only way in which we can reasonably account for this effect upon refrangibility is by supposing that the bright star consists of two separate stars so close together that there is not the slightest prospect that with even the most perfect optical instrument we could divide them into two separate objects. As these stars are revolving one around the other in a plane which, for our present purpose, we may regard as passing through the eye of the observer, it will happen, of course, that at certain parts of the path one of the stars is coming towards the Earth while the other is going from it. On the other hand, when the stars have advanced a quarter of a revolution further along their track, both of them will be moving athwart the line of vision. The spectra produced in these two cases will possess quite distinct characters. In the former case the lines interrupting the light emitted from the approaching star will have a refrangibility greater than that which naturally belongs to them, while the lines present in the light from the receding star will have their refrangibility lessened. The consequence will be that the lines from one of the stars are shifted towards one end of the spectrum, and the lines from the other undergo a shift towards the opposite end. No doubt the actual amount of such shift is comparatively small; indeed, the wonder is that it should be large enough to be in any degree appreciable. In certain cases, however, it does attain a measurable amount, and gives evidence of its presence by the remarkable doubling of the lines. When the two stars, on the other hand, are moving athwart the line of vision their proper movements have no effect on the apparent refrangibilities of the light they emit, and consequently the lines close up again. We can therefore explain the periodic doubling of the lines in the spectrum of Mizar by the simple supposition that what we take to be a single star consists really of two very close stars revolving one around the other.

But the information at our disposal enables us to learn a good deal more with regard to this interesting object. From the observation of the interval of time between the successive duplications of the lines, we discern the period in which the stars accomplish a complete revolution around their common centre of gravity. By measurements of the distance between the components of the double lines, we ascertain, further, the velocity with which one of the stars is coming towards our Earth, and the velocity with which the other star is retreating from the Earth; or, to put it rather more correctly, we ascertain the velocity of separation of the two bodies, even if, as is most likely the case, their two velocities be not actually equal. We have therefore learned with regard to the orbit of the components of this composite star, both the periodic time that is required for a complete circuit, and the velocity with which the movement is performed. Having, therefore, both the periodic time and the pace, we can determine the length of the journey, and thus we discover the dimensions of the orbit in which one of the stars revolves around the other.

This point being reached, the next step is a comparatively easy one. We are convinced that such movements as we have been considering can only arise in consequence of gravitation between the two masses, and the intensity of the gravitation depends upon the magnitudes of the masses. From our study of the solar system we have learned that there is a certain relation between the time a planet requires for a revolution round the Sun and the mass of the attracting body. If, for instance, the Sun were much heavier than it is, then the Earth would have to move round in its orbit in less time than a year if it were to preserve the distance now separating it from the centre of attraction. So, too, if the Earth possessed the mass of the Sun, the Moon would have to whirl round in the very brief period of a little more than an hour, instead of in twenty-seven days, if it were not to be drawn in from its present distance of 240,000 miles. From these illustrations it can be inferred that, when we know the time which a planet takes for its revolution, and the dimensions of the orbit in which it moves, we can compute the mass of the body by whose attraction its movement is governed. This argument has indeed been already alluded to in connection with the distance of the Sun. In the

PLATE XI.



NEBULÆ AS SEEN BY LORD ROSSE'S TELESCOPE.

case of the binary star now under consideration, it is not perhaps correct to say that we can actually determine the mass of each of the two components separately. But we are able to learn their united mass, and that is indeed a momentous result. It has been demonstrated that the observed movements of the lines in the spectrum of Mizar are inexplicable except on the hypothesis that this star, or rather its two components taken together, have a mass not less than forty times that of the Sun.

These considerations will enable us to appraise the position which the Sun occupies in the scheme of the universe. We have found that our luminary is indeed a star, one of that myriad host of which more than a hundred millions are in some degree accessible to our scrutiny. See, for instance, Mr. Russell's wonderful photographs of the nubeculæ in Figs. 80 and 81, and the nebulæ depicted in Plate XI. We have learned that the Sun takes rank as a star, of the same character as Capella, its radiance differing in certain noteworthy features from that dispensed by other bright stars. The Sun is hotter, apparently, than some stars, cooler, apparently, than others. The Sun is also star-like in the circumstance that it does not occupy a fixed position in space, but, like other stars, wends its way on a never-ending journey. We have seen, too, that it has been possible to compare the mass of the Sun with the masses of some other stars. There can be no doubt that the Sun must be heavier than many of the bodies which lie around us. We have, however, seen that it is certainly lighter than Mizar, and doubtless many other gems of the sky are far more ponderous than the central body of our system. From these various points of view we are led to regard our Sun as a star of no exceptional kind. It is remarkable neither for its movements, its lustre, its colour, its size, or its weight. Whatever may be its importance to our welfare, it is, nevertheless, as has been well said, only "a private in the host of heaven."

CHAPTER XVIII.

THE MOVEMENTS OF THE SOLAR SYSTEM.

IN this chapter we have to enter into an account of one of the most daring exploits ever performed by astronomers in their attempts to explain the mysteries of the heavens. We have already shown how the various planets, with their satellites, constitute a group clustering around the Sun, by whose attraction their movements are controlled. What we have now to consider are the several circumstances connected, not with the individual members of this group, but with the group itself as a whole. In this connection we may speak of the solar system as a small object; for, in dealing with conceptions such as must now occupy us, the system of bodies circulating around the central luminary is altogether lost sight of, the Sun itself being regarded as a star no brighter than any one of the many hundred thousand gems that nightly adorn our skies. We have, in fact, now to treat the Sun as a subordinate body in the host of heaven, and not as the centre of light and life to a group of attendant worlds. The various points of resemblance and the various points of contrast between the star that we know as the Sun, and certain other stars throughout the universe, have been already set forth. What we have now to consider relates not at all to stars in their capacity of light-radiators; we have merely to discuss certain primary facts connected with their movements in the heavens.

That a body can remain at rest in the universe is in the highest degree improbable. The first law of motion declares that a mass, once set in movement, will continue for ever to travel onwards in a straight line with unabated velocity, except in so far as other forces may intervene to affect it. If, therefore, an object be started on a voyage through empty space, it is almost impossible that it could ever be brought to actual rest by any combination of circumstances. We must be cautious not to allow our experience of movement on the Earth to influence unduly our judgment as to what would happen in open space. We know so well that the train soon comes to a standstill when the steam has been shut off, that we are tempted to think of movement as apt to die out of itself. But in all such cases, closer examination will show that the decay of the movement is to be attributed to the action of retarding forces. The train is brought to a standstill because friction against the rails and the resistance of the air operate to destroy the velocity with which it was animated. The first law of motion assures us that if a train could move without friction, and if there were no resisting medium to exercise its influence, its movement would continue unabated for ever on a level road, even though the steam were wholly shut off. Though we have no opportunity in our actual terrestrial experience of putting this law to the direct test, there are so many methods by which we can convince ourselves of its truth that no one hesitates to accept it.

In astronomy this first law of motion rises to a principle of the utmost importance. Out in open space a moving body experiences no friction, nor is its movement diminished by the resistance of a medium. Hence it follows that every object, whether it be as minute as a mote in the Sunbeam, or as large as a majestic Sun, would continue to move on for ever with unabated speed, provided, I ought perhaps to add, that all the other bodies in space happened to be so remote as to exercise no appreciable disturbance by their attraction. It is thus obvious that a body once set in motion will never return to rest; and as there are agents always in operation whereby stellar movement may originate, is it surprising that all the objects known to us in the universe should be regarded as being in motion? In fact, it is easy to see that if a body were poised at rest for a moment in open space it could not remain there; the attractions of other distant bodies, feeble though they might be, would commence to operate, and would speedily impart movement to the once quiescent mass. We ought, indeed, also to add that we have no means whatever of recognising whether a body is absolutely at rest or not. The stars, which are our only landmarks in space, cannot here help us, for if the stars themselves be in motion, as we know they are, how can they be employed to test the quiescence of objects with which they are compared? Indeed, it seems infinitely improbable that there should be any fixed material particle in the whole universe.

But we must first remove what would otherwise appear to be a contradiction. We expressly designate the brilliant gems which form our constellations as "fixed stars," in contradistinction to the planets or wandering star-like bodies. No doubt the stars seem fixed, as far as casual observation goes, when we view them from year's end to year's end, and even in the course of centuries most of them seem to have suffered but little displacement. It seems certain that even in thousands of years the lineaments of our familiar constellations have not appreciably altered. But change is the law of the universe. Even the great mountains have no permanence when periods of geological time are considered. The constellations in the heavens above are as mutable as the configuration of the land beneath. Strictly speaking, there are no fixed stars, for though they may seem fixed to our ephemeral gaze, yet, in regard to the vast duration of time with which the study of nature makes us acquainted, it is plain that the whole universe is in motion.

Enormous periods should, however, elapse before any extensive dislocation of the apparent disposition of the stars could result from their present velocities. But there cannot be a doubt that when we take such long ages into view as those which geologists declare to be necessary for the accomplishment of the transformation of our globe; when we think, for instance, of the many millions of years which must have elapsed since the great coal forests flourished, it is clear that there must have been time for a total transformation of the appearance of the heavens. If a man had been living at that remote epoch, it is extremely probable that the stars which he would have seen in the heavens which arched over his head would be totally different from those adorning our present skies. The same planets would be there, no doubt; their incessant changes do not permanently remove them. It is the so-called fixed stars which would have undergone transformation. Not alone would the lineaments of the constellations have been deranged; even the groups would not be recognisable—nay, the very stars themselves would be different. It is, indeed, quite probable that not one of the stars now conspicuous in the sky would have been visible to an observer at the remote date I have mentioned; while, on the other hand, multitudes of stars might have been seen by him which have long ceased to be within terrestrial ken. When, after the lapse of myriads of years, the coal ages had passed away, and our globe had become gradually transformed into the Earth we now know, a new heaven looked down upon a new Earth.

The reason why the stars seem to move so slowly is that they are so excessively remote. Were we close enough to a star to gauge by ordinary standards the speed at which it was hurrying along, we should find that so far from being so sluggish as to require centuries for its manifestation, its movement was excessively rapid. The Earth moves in its track around the Sun at a pace of about eighteen miles a second. We may take this as a standard by which to estimate the velocity of other large celestial bodies. We find that the pace with which many of the stars move is quite as great as that with which the Earth urges its way. In some cases, indeed, the stellar velocity is far more than eighteen miles a second; there are instances in which it amounts to as much as forty miles a second; and paces far greater still are not unknown. The star known to astronomers as Groombridge 1830 hurries along, as we have already seen, with a pace certainly not less than 200 miles a second. These movements, though intrinsically so prodigious, nevertheless appear small to us on account of the stupendous distances at which the stars are situated. It is, however, easy to show that even though Groombridge 1830 moves with a velocity more than ten times that of the Earth in its orbit, its displacement is still inconsiderable when viewed from the distance at which we are placed. We know it to be about a million times as remote from us as the Sun, and consequently a simple calculation will suffice to show that even though the star be moving at a pace of 200 miles a second, it would occupy a period of about 250 years in traversing an arc of the sky no greater than that which the diameter of the Moon presents to the eye. And yet a change in the position of a star to the extent of the Moon's diameter would not be very noticeable. Only by careful allineation with the surrounding stars could it be rendered distinguishable by the eye. If the most rapidly moving star only effects so small a change of place in a period of 250 years, need we be surprised to find that ordinary stars do not move quickly enough to affect their positions appreciably, even within thousands of years?

I have in these remarks referred merely to observations made with the unaided eye. When we call to our aid the instruments of the observatory, we are enabled to infuse into our observations a precision that permits us to recognise extremely minute quantities. By that most perfect of measuring instruments, the meridian circle, we can locate the place of a star on the sky with such exactitude that the movements of the stars, small though they doubtless are, can be recognised even within brief intervals of time. With how great delicacy one of these telescopes can be pointed, may be inferred from the following illustrations. Suppose that an object no larger than a skylark, six inches long, were at an altitude of eighty miles, and that it were viewed through a telescope provided with those elaborate measuring appliances that are proper to the meridian circle. Even at such a vast altitude the difference in the position of the beak of that skylark and the tip of its tail would be distinctly appreciable to an observer provided with the resources of modern astronomy. The point on the heavens indicated by a line from the observer's eye to the tip of the bird's beak, and the point on the heavens indicated by the line from the observer's eye past the tip of the tail, would be capable of having their angular distance measured.

With such refined appliances at our disposal for making accurate determinations, we are enabled to detect the movements of many stars, and to ascertain their angular rate. A year is quite long enough for the detection of changes in the positions of the more rapidly moving stars. Indeed, in some cases we find that in a few months, or even in less, the proper motion of the star is sufficient to cause a derangement of its place on the sky quite perceptible to our refined method of inquiry.

It should, however, be remarked that, notwithstanding the accuracy of our meridian work, the great majority of the stars which have been hitherto observed do not disclose any certainly recognisable proper motions. We do not doubt that stars are invariably animated by certain movements; but in a great many cases these movements are so slow that we are unable to recognise them from our remote standpoint. It must also be borne in mind that in our observations with the telescope we can only take cognisance of that component of the star's movement which is directed along the surface of the heavens—that is to say, perpendicularly to the line of sight. If, for instance, a star happened to be coming directly towards the observer, then the telescope would give no indication of such movement, because the apparent place of the star on the sky would not be altered. And the same remark, of course, also applies to the star flying directly away from the Earth. No doubt in both these cases some alteration in the apparent lustre of the star would be the consequence of a movement of this character. The star which was approaching should increase in apparent brightness, while the apparent brightness of a receding star should wane. But such changes in lustre would be quite inappreciable by any means at our disposal even after the lapse of a century. It would, indeed, generally happen that the movements of the star would be directed neither wholly in the line of vision, nor wholly perpendicular thereto. The proper motion which we perceive is, therefore, only the projection of the star's movement on the face of the heavens. It is thus obvious that the velocity of a star obtained by such telescopic observations as those we have described must, generally speaking, be less than the actual rate of the movement with which the star is animated. The spectroscopic method is, however, now available for measuring that component of the movement which is directed along the line of vision. How that method is applied, and some of the results to which it has led, have been discussed in the last chapter.

We see from such observations that it is characteristic of a star to possess a certain movement of translation, and this prepares us for the consideration of the great problem which we have now to discuss. We know that the Sun resembles a star in the matter of light and heat; we also know that many of the stars are comparable with our Sun, both in mass and in dimensions. Shall we say that the resemblance between these different bodies stops at this point? We must rather push it one step further, and as we find that the stars are generally characterised by movement, might we not expect that the Sun would also resemble a star in the possession of a proper motion through space? When we consider all that is really implied, this is a grand problem indeed. In discussing it we must think of the Earth merely as an appendage to the Sun—in fact, just as the Moon accompanies our globe in its annual voyage round the Sun, just as Jupiter performs his majestic circuit with his little attendant moons circling round him, so, if the Sun is in movement at all, it must bear with it the Earth and all the other planets. The question is not devoid of a certain human interest—at all events, to this extent: that, wherever the Sun may go, to whatever part of the heavens his voyage may be directed, thither do we accompany him.

But the solution of the problem is surrounded with many difficulties. How are we to discover the displacement of the Sun in space? for it is to be noted that any direct observations which we can make of the Sun itself cannot avail. We accompany the Sun in his journey; as he moves, so do we, and consequently there can be no apparent displacement of the luminary, to tell us what we want to know. In like manner, an inhabitant of the Moon could never learn by mere observation of the Earth, as he saw it from his point of view, that it was in revolution round the Sun; he would require to compare it with other points of reference. Doubtless the lunarian would find the Sun itself a convenient object of comparison by which to establish that movement of the Earth in which his own orb participated. But we have no point of reference of that kind. If we could remove ourselves from the Earth to some independent position in space from which we could observe our Sun with the meridian circles and other instruments of the observatory, then we might determine the proper movement of the Sun in much the same way as that in which we discover the proper movement of a star. But this course is denied to us. How, then, are we to discover the character of a movement shared by the Earth?

The points of reference that we must employ can be provided only by the stars. Observations of the planets would of course be utterly useless for our present purpose, because the planets, like the Earth, share in the movement of the system. Nor are there any other landmarks through the universe which could possibly avail. We are compelled to resort to a comparison between the Sun and the stars. But here we are encountered by a fundamental difficulty. We can judge of the rate at which a carriage is drawn along a road by the intervals at which the consecutive milestones are passed. But how far would such a process be available if it should happen that the milestones were themselves in motion? How could the captain of a vessel at sea gauge the motion of his craft if he had no points to compare with his position save those provided by the other vessels sailing around him? The determination of the speed of the vessel would be open to no little uncertainty. That is, however, the precise difficulty which the astronomer experiences in his attempt to detect the movement of the Sun by the help of the stars. For as the stars are themselves in motion, it might seem useless to appeal to them as points of reference whereby to exhibit the movement with which another star is affected. There can be no doubt that if we were restricted in our investigation to a few stars only, the solution of the problem would be well-nigh impossible; the proper movements of these stars and the movement of the Sun would be blended inextricably. No doubt the joint result of the genuine stellar movement and of the apparent stellar movement arising from the displacement of the Sun would be appreciable, but it would seem impossible to discriminate between the contribution to the net result made by the actual displacements of the stars, and that which was due to the movement of the Sun. It is, however, fortunate that we are not restricted to a few stars in the pursuit of this inquiry. There are thousands of stars offering their aid in the research, and in this instance at all events there is safety in a multitude. We know how important are the principles which can be deduced from the law of averages. What can be more uncertain than the duration of the life of a human being? But when humanity is dealt with by the thousand, the uncertainty so characteristic of the individual disappears. Insurance companies conduct a vast business in the confident belief that, however precarious may be the life of any single individual, yet the laws of mortality hold with inflexible regularity when a sufficient number of individuals are dealt with at the same time. It is this circumstance which enables us to employ stars as marks for the purpose of solving our present problem. No doubt each one of those bodies possesses more or less of proper motion. But here comes in the application of that principle of averages to which I have referred.

Let us assume that every star moves independently of all the rest. Let us assume that there is no general disposition of the stars to move up more than down, to the right more than to the left, with one velocity more than with another. Let us consider, in short, the velocities with which all the stars are animated as absolutely irregular; then they may be safely employed for the purpose of our investigation. It might at first sight appear that perfectly random movements of the stars would present exactly the conditions that were most unsuitable; but the opposite is the case. The real danger of arriving at a futile result would only arise if the stars manifested a set in any particular direction. For example, let us suppose, as a very extreme case, that all the stars happened to move precisely in parallel lines, and that all happened to move with exactly the same velocities. We should then have materials for a deduction as to the movement of our Sun which would be certainly erroneous. Our methods would lead us to conclude that the consentaneous movements of the stars were only apparent, and that what was really in motion was the Sun, with a velocity equal in amount to that of the stars, but in the opposite direction. Even if the stars did not all move with those precisely similar movements I have supposed, yet, if it so happened that they manifested a certain tendency or drift, so to speak, in one direction, the inferences which would be drawn from the application of our only available method would be correspondingly untrustworthy. Our hope for a successful solution of the problem lies in the validity of our assumption that the stars selected for the purpose of the investigation are animated only by what we can call random movements, without a bias in any particular direction. In such a case we estimate that when a sufficient number of stars have been dealt with, the set of a certain number of them in any direction is on the average compensated for by the set of a nearly equal number in the opposite direction. We assume, too, that the speeds with which the stars move in one direction are, on the whole, nearly equal to those with which they move in the opposite direction. If these suppositions are not justified by the fact, then the results which we obtain are correspondingly unreliable. All we can certainly say is, that no other method is available to us for studying the motion of the solar system than that which we have just indicated.

Assuming that the movements of the stars are sufficiently conformable to the specified conditions, let us see how they can be rendered available to solve the problem that lies before us. We may now forget the proper movements of each individual star. We shall regard them merely as beacons at rest in the universe, and we shall inquire what changes in their appearance should be presented if our Sun were the only moving body. We may set aside as altogether beyond the reach of our discovery the detection of the form of the orbit in which the Sun possibly revolves. It is, of course, conceivable that the Sun may be performing a journey in a curve of some description in its passage from some unknown region of space to some other equally unknown destination. It would, however, be perfectly futile for us to speculate on what such a curve could be. Doubtless, if our observations had extended over some millions of years, we might be able to discover something more or less accurately with regard to movements so stupendous. But with the limitation imposed by the fact that precise observations have only been attainable during one century, we can discuss no more than the actual rectilinear movement of the Sun at the present moment. As a mathematician would say, the Sun makes its journey in a curve of some sort, and at every instant it may be regarded as moving with a certain velocity in the tangent to that curve. We may never be able to find out the curve itself, but what we do hope to find out, and what, in fact, we have succeeded in finding out with some tolerable degree of completeness, is the direction of the tangent to the curve at the point in which the Sun is now situated, and the velocity with which the Sun moves in that direction. This is, so far as it goes, a useful contribution to our knowledge. Mathematics, however, declare that as the Sun is attracted by other stars, the solar motion cannot be really performed in a straight line. It must, therefore, be executed in some curve or other, of the shape or direction of which we can observe nothing, and the nature of which we are unable to calculate, inasmuch as the character of the attracting forces by which that curve must be determined are almost entirely unknown.

To illustrate the process which is employed in the investigations of the movement of the solar system, I shall consider some simple facts connected with the movement of a ship at sea. Suppose that a sailor is drawing near a harbour at night, and that at the mouth of the harbour there are two lighthouses to mark the entrance, one on the right hand and the other on the left. When, from a long distance off, the navigator first discerns those beacons they seem close together, so that, from his point of view, the angle is a small one by which the two lights appear to be separated. Suppose, for instance, that the lights are half a mile apart while the navigator is ten miles away, then the angle between the lights will be only about three degrees. I use this method of measurement because, as will be seen when we deal with the problem presented on the surface of the heavens, it is the angular distance between two stars which forms the object of our determinations. When the vessel has approached within five miles of the mouth of the harbour, the apparent distance between the two lights has plainly widened; the angle which they then subtend has increased to about six degrees; and as a still closer approach is made, it gets larger and larger, until, at a distance of about a mile from the mouth, the beacons are thirty degrees apart. As the moving body, in short, approaches the two lights, each of which is at rest, the angle subtended by them gradually increases. On the other hand, if the navigator were leaving the harbour, he would find that the very large angle subtended by the lights just after he had passed the entrance, became less and less according as his vessel became more and more remote.

It is thus possible to imagine a method by which the sailor could judge of his movements from the successive alterations in the apparent angular distances between the two lights. If the direction in which he was sailing were such as to lead him straight into harbour, then the increase in the angular separation alone would acquaint him with that fact. By repeating the observation a little later, it would be quite possible for him to determine, with all necessary accuracy, both the number of knots per hour at which his vessel was moving, and the direction in which his course lay. This is substantially the method on which the astronomer has to rely when attempting to solve such questions as those presented by the movement of the solar system through space. In this great investigation the astronomer may be regarded as viewing the phenomena from the Sun itself; for the Earth on which he stands accompanies the Sun in its movements, and the distance from the Earth to the Sun is quite insignificant compared with the vast scale of our present inquiries. The stars are the beacons to be employed, and for our present purpose we may regard them as divested of their individual proper motions. In the illustration that we have given of the movements of the ship, the lights seemed to diverge from the point towards which the direction of the movement was urged. We might therefore expect that if the Sun were bound on a great voyage through space, the starry beacons which extend all over the sky should testify by their apparent displacements, to the actual movements with which the Sun was animated. The stars should thus seem to travel along radiating lines gradually away from that point of the sky towards which the Sun's movement is directed. We might therefore hope to discover the goal of the solar system by simply ascertaining the whereabouts of that point on the celestial sphere from which, as observation showed, the stars generally seemed to recede.

It would be natural to test the soundness of this method of investigation by examining whether that which ought to happen at the opposite part of the heavens, if our supposition were true, actually does happen. If the Sun be advancing towards one point of the celestial globe, it must be retreating from the diametrically opposite point. Our illustration of the movement of the ship has demonstrated that if such a retreat be in progress, the stars ought to manifest a convergence towards the point retreated from. We should thus find that there were two distinct classes of phenomena to be looked for which, if found, would severally corroborate our solution of the problem. If it should appear that there was one point in the sky towards which the stars seemed to draw in, and if it were also found that from the opposite point of the celestial sphere the stars appeared to be spreading away, then it might be regarded as perfectly established that the movement of the Sun was along the line thus indicated.

Many astronomers have undertaken the laborious task of investigating the movements of the Sun in accordance with the principles I have explained. One of the most recent of these determinations, as well as one of the most elaborate, is that by Professor Oscar Stumpe, in which he has obtained from a large number of stars conclusive evidence as to the whereabouts of the point in the heavens towards which the solar system is advancing. It must, of course, be remembered that each of the stars is in movement, so that the displacements apparently affecting them have a composite origin. They are due to stellar motions properly so-called, blended with apparent shifts of position in virtue of that motion of the solar system which is the object of our particular research. If the latter be the main source of movement, then we might expect that a considerable part of the observed proper motions should admit of explanation according to the rules of perspective above laid down. And it has, in fact, been found that a large part of the observed proper motions of the stars can be accounted for on the supposition that the Sun does really move towards a certain point in the sky, which has been determined with some certainty. Of course we cannot claim for this conclusion the force of absolute demonstration; but if we refuse to accept it, we must admit what is much more difficult of explanation—namely, that all the stars combine to exhibit a consentaneous movement of the kind actually observed. In the course of his researches, Herr Stumpe constructed a list of all the stars whose movements had been accurately determined by the best astronomical authorities. The total number of those investigated by him is 1,054. These are widely distributed over the heavens, the object being, of course, to eliminate as far as possible anything like the systematic errors which might have been expected to occur if the stars had been chosen from a limited region. It should, however, be noticed that the stars employed in this investigation are chiefly confined to the northern hemisphere. This will perhaps not be surprising when it is remembered how much more copious are the available observations of the stars in the northern hemisphere than in the southern. However, Herr Stumpe carefully brought together whatever suitable stars from the southern hemisphere were to be found in catalogues of indisputable authority. I should mention that he has not included in his list any star unless the proper motion which it possessed was at least sufficient to carry it in the course of 10,000 years over a distance on the heavens as great as the apparent diameter of the Moon.

His first task was to classify these stars into different groups according to the amount of proper motion which they exhibited. The first group, to the number of 551, was composed of those which had proper motions between the lower limit and an amount double as much—that is, ·32 of a second. The second group of stars, to the number of 340, had each a proper motion lying between ·32 and ·64 of a second. The third group, numbering 105, was composed of stars whose proper motion was from ·64 up to 1·28 of a second; while the last group of most rapidly-moving stars numbered fifty-eight, and included all stars with proper motions exceeding those belonging to the lower classes. Herr Stumpe attacked the problem by investigating the movement of the Sun which would best explain the proper motions of the stars in the first group; he then applied similar inquiries to each of the three other groups. It will be noted that as each of the stars in one group is wholly independent of the others, this investigation practically provides four entirely distinct determinations of the apex of the Sun's way. It is of much interest to examine whether the several points indicated by these four different lines of research are practically coincident. This, of course, ought to be the case, if the result to which they lead is correct. For example, if it appeared that the apex suggested by the first of these groups was widely different from that indicated by any of the others, it would then have been impossible to have drawn any satisfactory conclusion from the results. But if the four points to which we are thus independently conducted should all be found located in the same neighbourhood in the heavens, then, indeed, we seem warranted in drawing the inference that we have solved the problem.

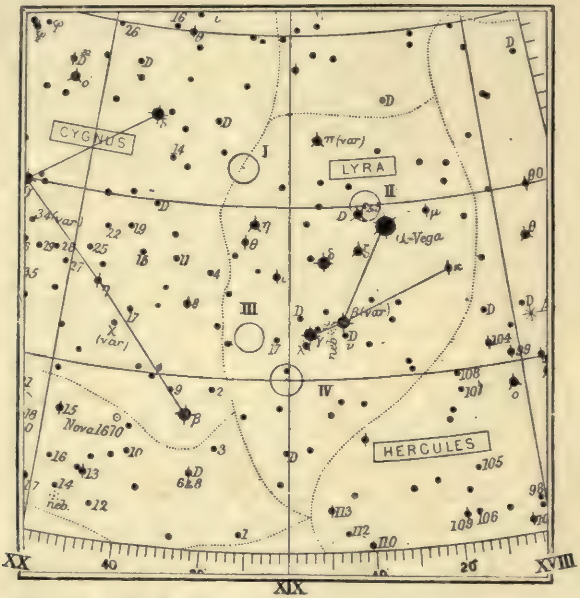


FIG. 82.—THE SEVERAL DETERMINATIONS OF THE POINT TOWARDS WHICH THE SOLAR SYSTEM IS TENDING.

It has been shown in Fig. 82 how all the points are contained within a small patch of the sky in the constellation of Lyra lying between Cygnus and Hercules. The position indicated by the first group of stars, being those whose proper motion is least conspicuous, lies just on the confines between Cygnus and Lyra. The second group gives an apex with a very remarkable position, inasmuch as it is situated quite close to the beautiful star, Vega, the most brilliant gem of the northern hemisphere. The third and fourth groups correspond with points in the southern parts of Lyra. The mean of the four positions falls obviously at no great distance from Delta Lyræ. So far as our present knowledge goes, it seems therefore certain that the apex of the Sun's way cannot be remote from the star just named. It is impossible to avoid making a remark suggested by the proximity of the constellation Cygnus to the point thus determined. We know that the star numbered 61 in that constellation is the nearest to the Earth of all those stars in the northern hemisphere whose distance has been as yet measured. We now see that this is the part of the heavens towards which the solar system appears to wend its way. Is it possible that there can be a connection between these two facts? No doubt the approach of the Sun towards the part of the heavens occupied by Cygnus would tend in the lapse of ages to reduce the distance between the Sun and the stars of that constellation. Can it be that the nearness of 61 Cygni is in part a consequence of the solar advance during the ages of the illimitable past?

I have referred with some detail to these researches, because they are the most comprehensive of any which have been undertaken up to the present. I should, however, add that several astronomers have investigated the same great question from time to time with results which, speaking generally, agree as closely as could be expected with those which we have specially set forth. Mr. Lewis Boss, the capable American astronomer, has recently published an investigation of the solar apex, in which he has availed himself of a good deal of new material, and varied the manner of its treatment. The stars employed by him are all contained in one zone, about four degrees in breadth, just north of the celestial equator. Considering the very limited area of the heavens to which Boss's researches referred, and considering also that of the stars which he employed only one-fifth were known to have been previously used in a similar inquiry, it is extremely interesting to note that the apex to which his calculations conducted him lies very near those which had been already indicated by Stumpe's much more elaborate investigation. Mr. Boss's final result places the apex quite close to Vega. Indeed, it is almost the same as the point marked II in Fig. 82.

A further investigation of this problem may be mentioned, as it is the most recent with which I am acquainted. Mr. J. C. Porter has deduced the movement of the solar system from the proper motions given in a catalogue of 1,340 stars, forming Publication No. 12 of the Cincinnati Observatory. In this research the author followed Herr Stumpe in his classification of the stars into four groups corresponding to the magnitudes of their several proper motions. The results arrived at are remarkably near those obtained by Herr Stumpe, excepting that the declination of the point indicated by the first group is somewhat greater. Considering how concordant are the results obtained from different investigations which have all the merits of independent determinations, it seems hardly possible to doubt that the Sun must be really moving in the direction which we have indicated.

One other observation must be made in conclusion. We have been engaged in determining what we call the movement of the Sun through space, but we ought certainly to qualify what has been said with the remark that, after all, the movement with which we have been concerned is only a relative one. As to the absolute movement of the system through space, it does not yet seem possible for us to learn anything. All that we have attempted is to determine the path which the Sun takes through that group of stars which surround us. It is practically certain that relatively to these stars the Sun has the movement which the research has indicated. But this is a statement widely different from an assertion that what we have discovered represents the absolute movement of the Sun through space. We must remember that all the stars visible around us only form a group in its infinite extent. If we could look on the sidereal system from some external point which would enable us to view sidereal phenomena in their proper perspective we should find that the group to which our Sun belonged, in so far as it consisted of stars discernible by us, was a limited one. This must be so, even though the group included all the stars visible through our telescopes; even though it included all the stars in the Milky Way; even though it included the faintest point of light that ever succeeded in imprinting its image on the most sensitive photographic plate after hours of exposure. This unnumbered host is still only a cluster, occupying, comparatively speaking, an inexpressibly small extent in the ocean of infinite space. Looked at in this way, we are able to place the results which have been arrived at in the course of this chapter in their proper proportion. We have been able in some measure to follow the track of our Sun as it cleaves its way from century to century through the cluster. But even still it remains in the cluster, and the cluster itself is, as we have seen, only a speck in infinite space. It may be asked whether the cluster may not be in motion as a whole? In fact, may not all those stars which we have used for our investigations themselves participate in a colossal movement, shared in by the Sun among the rest? Probabilities incline us to the belief that this must be so. Just as the Earth and all the other planets accompany the Sun in its voyage towards the limits of our cluster, so the Sun, with the other stars belonging to that cluster, are not improbably animated by some vast motion which affects them all alike. The imagination will carry us further still. It will show us that our star cluster may be but a unit in a cluster of an order still higher, so that a yet higher possibility of movement is suggested for our astonishment. Great as is the problem we have attempted to solve, we now see that it is only a small part of a far grander and more comprehensive problem, the solution of which lies totally beyond our reach.

[*Reproduced from "Story of the Heavens."*]

APPENDIX.

ASTRONOMICAL QUANTITIES.

THE SUN.

THE Sun's mean distance from the Earth is 92,700,000 miles; his diameter is 865,000 miles; his density, as compared with water, is 1·4; his ellipticity is insensible; he rotates on his axis in a period between 25 and 26 days. His diameter is 109 times that of the Earth; and his mass about 330,000 times that of the Earth. His mean angular semidiameter is 16 minutes of arc.

THE MOON.

The Moon's mean distance from the Earth is 238,000 miles. The least possible distance is 221,000 miles; the greatest is 260,000 miles. The diameter of the Moon is 2,160 miles; and her density, as compared with water, is 3·5. The time of a revolution around the Earth is 27·322 days.

THE PLANETS.

	Distance from Sun in Millions of Miles.			Periodic Time in Days.	Diameter in Miles.	Axial Rotation.			Density Compared with Water.
	Mean.	Least.	Greatest.			h.	m.	secs.	
Mercury	35·9	28·6	43·3	87·969	2,992	Uncertain.			6·85
Venus	67·0	66·6	67·5	224·70	7,660	Uncertain.			4·81
Earth	92·7	91·1	94·6	365·26	7,918	23	56	4·09	5·66
Mars	141	128	155	686·98	4,200	24	37	22·7	4·01
Jupiter	482	459	505	4,332·6	85,000	9	55	—	1·38
Saturn	884	834	936	10,759	71,000	10	14	23·8	0·75
Uranus	1,780	1,700	1,860	30,687	31,700	Unknown.			1·28
Neptune	2,780	2,760	2,810	60.127	34,500	Unknown.			1·15

THE SATELLITES OF MARS.

Name.		Mean Distance from Centre of Mars.		Periodic Time. hrs.	mins.	secs.
Phobos	...	5,800 miles	...	7	39	14
Deimos	...	14,500 „	...	30	17	54

THE SATELLITES OF JUPITER.

Name.		Mean Distance from Centre of Jupiter.		days.	Periodic Time. hrs.	mins.	secs.
New Inner Satellite (Barnard) ...		112,400 miles	...	—	11	50	—
I.	...	262,000 „	...	1	18	27	34
II.	...	417,000 „	...	3	13	13	42
III.	...	664,000 „	...	7	3	42	33
IV.	...	1,170,000 „	...	16	16	32	11

THE SATELLITES OF SATURN.

Name.		Mean Distance from Centre of Saturn.		days.	Periodic Time. hrs.	mins.	secs.
Mimas	...	118,000 miles	...	0	22	37	27·9
Enceladus	...	152,000 „	...	1	8	53	6·7
Tethys	...	188,000 „	...	1	21	18	25·7
Dione	...	241,000 „	...	2	17	41	8·9
Rhea	...	337,000 „	...	4	12	25	10·8
Titan	...	781,000 „	...	15	22	41	25·2
Hyperion	...	946,000 „	...	21	7	7	40·8
Japetus	...	2,280,000 „	...	79	7	54	40·4

THE SATELLITES OF URANUS.

Name.		Mean Distance from Centre of Uranus.		Periodic Time. days.
Ariel	...	119,000 miles	...	2·520383
Umbriel	...	166,000 „	...	4·144181
Titania	...	272,000 „	...	8·705897
Oberon	...	363,000 „	...	13·463269

THE SATELLITE OF NEPTUNE.

Name.		Mean Distance from Centre of Neptune.		Periodic Time. days.
Satellite	...	220,000 miles	...	5·87690

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